THE AMERICAN PHILOSOPHICAL SOCIETY
BICENTENARY CELEBRATION
PHILADELPHIA
1727–1927

THE RECORD OF THE CELEBRATION

OF THE

TWO HUNDREDTH ANNIVERSARY

OF THE FOUNDING OF THE

AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA FOR PROMOTING
USEFUL KNOWLEDGE, APRIL 27
TO APRIL 30, 1927

PROCEEDINGS
VOLUME 66

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PREFACE

At the General Meeting of the American Philosophical Society held on April 27–30, 1927, the Two Hundredth Anniversary of the Founding of the Society was Celebrated.

In addition to the regular communications the Committee on the Bicentenary Celebration invited ten eminent scholars to present special papers covering various fields of Science.

Societies and other Institutions of Learning at home and abroad were invited to send Delegates and present Addresses.

This volume is a record of the "Proceedings."

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THE PROGRAMME

THE PROGRAMME

WEDNESDAY EVENING, APRIL 27TH

At the Residence of
COUNCILMAN J. BERTRAM LIPPINCOTT
Council Meeting

THURSDAY MORNING, APRIL 28TH

In the Hall of the Society on Independence Square
Executive Session, 10 A.M.

For the Transaction of the Private Business of the Society

MORNING SESSION, 10.30 A.M.

Reception of Delegates

THURSDAY MORNING, 11 A.M.

In the Hall of the Society on Independence Square

Meeting for the Reading of Papers on Subjects of Science

THURSDAY AFTERNOON, EXECUTIVE SESSION, 2 P.M.

For the Transaction of the Private Business of the Society

STATED BUSINESS—Election of Officers, Councillors and Members

2.30 P.M.

In the Hall of the Society

Meeting for the Reading of Papers

FRIDAY MORNING, APRIL 29TH

In the Hall of the Society

Meeting for the Reading of Papers

FRIDAY AFTERNOON

In the Hall of the Society

Meeting for the Reading of Papers

FRIDAY EVENING

RECEPTION

In the Hall of the Historical Society of Pennsylvania

RECENT DISCOVERIES RELATING TO THE ORIGIN AND ANTIQUITY OF MAN

BY HENRY FAIRFIELD OSBORN

SATURDAY MORNING, APRIL 30TH

In the Hall of the Society

Meeting for the Reading of Papers

SATURDAY AFTERNOON

In the Hall of the Society

Meeting for the Reading of Papers

SATURDAY EVENING

AT THE BELLEVUE-STRATFORD
Broad and Walnut Streets

7.30 P.M.
Farewell Dinner

LIST OF DELEGATES

LIST OF DELEGATES

Université de Paris—Twelfth Century
Monsieur Léon Robin
Professor Jules Drach

Regia Università degli Studi, Bologna—Twelfth Century

University of Oxford—Twelfth Century
Professor Herbert H. Turner
Professor Charles R. Morris

Cambridge University—Twelfth Century
Dr. A. N. Whitehead
Professor Ernest W. Brown

Universität Heidelberg—1386

University of St. Andrews—1411
President Nicholas Murray Butler

University of Glasgow—1451
Professor Graham Lusk

University of Edinburgh—1582
Professor H. J. C. Grierson

Reale Accademie Nazionale dei Lincei (Rome)—1603
Mr. Henry S. Washington

L'Académie des Sciences de Paris—1629
Professor Albert A. Michelson

Harvard University—1636
Mr. Edgar C. Felton

Royal Society of London—1645
Professor Ernest W. Brown

Imperial German Academy of Naturalists, Halle—1652
Professor Johannes Walther

Yale University—1701
Professor Charles Schuchert

Berlin Academy of Science—1702
Professor Albrecht Penck

Kongliga Vetenskaps Societeten—1710

Georg August Universität, Göttingen—1737
Dr. Phil. Goetz

University of Pennsylvania—1740
President Josiah Penniman

K. Danske Videnskabernes Selskab—1742

Royal Society of Arts, London—1754
Professor John Uri Lloyd

Columbia University—1754
Professor Michael Pupin

Brown University—1764
Professor Albert D. Mead

Kongliga Vetens Rapsoch Vetterhets Samhället, Göteborg —1778

American Academy of Arts and Sciences—1780
Professor William Duane

Literary and Philosophical Society of Manchester—1781

University of the State of New York—1784
Professor Frank P. Graves
Mr. Charles C. Adams

College of Physicians, Philadelphia—1787
Dr. Hobart A. Hare
Dr. John H. Gibbon
Dr. O. H. Perry Pepper

University of Pittsburgh—1787
Dr. Mont R. Gabbert

The Linnean Society—1788
Professor William B. Scott
Professor Oakes Ames

University of Vermont—1791
Professor E. G. Spaulding

Massachusetts Historical Society—1791
Mr. Lawrence Shaw Mayo

Royal Institution of Great Britain—1800
Professor Joseph S. Ames

Library of Congress—1800
Mr. Frederick E. Brasch

Royal Philosophical Society of Glasgow—1802

United States Coast and Geodetic Survey—1807

American Antiquarian Society—1812
Mr. Thomas Willing Balch
Professor Albert Bushnell Hart

Academy of Natural Sciences of Philadelphia—1812
Mr. R. A. F. Penrose, Jr.
Mr. T. Chalkley Palmer

New York Academy of Sciences—1817
Professor Henry F. Osborn
Professor Nathaniel L. Britton
Professor Michael I. Pupin
Professor James McK. Cattell

Cambridge Philosophical Society—1819
Professor E. W. Brown
Professor Henry F. Osborn

Académie de Medicine de Paris—1820
Professor W. S. Thayer

Societas Pro Fauna et Flora Fennica—1821
Dr. John W. Harshberger

McGill University—1821
Dr. Archibald McCallum

Amherst College—1821
Professor George Olds

British Association for Advancement of Science—1822
Professor A. N. Whitehead

Rhode Island Historical Society—1822
Mr. G. Alder Blumer

Royal Asiatic Society—1823
Professor Charles R. Lanman
Professor A. V. Williams Jackson

Franklin Institute of Pennsylvania—1824
President W. C. L. Eglin
Dr. Howard McClenahan

Connecticut Historical Society—1825
Dr. George C. F. Williams
Rev. Arthur Adams

Jefferson Medical College—1825
Dr. Albert P. Brubaker
Professor J. Parsons Schaeffer

Magyar Todományos Akadémia, Budapest—1825

University of Toronto—1827
Dr. Thomas McCrae

Royal Geographical Society, London—1830
Mr. Frederick Watson

Boston Society of Natural History—1830
Professor George H. Parker

University of London—1836
Professor A. N. Whitehead

University of Michigan—1837
Professor Wm. B. Hobbs
Professor Moses Gomberg
Professor Bradley M. Davis

University of Missouri—1839
Mr. James C. Lawrence

American Statistical Association—1839
Mr. George Bedinger
Professor Stuart A. Rice
Dr. I. M. Rubinow

American Oriental Society—1842

Maryland Historical Society—1844
Mr. Lawrence J. Morris

Smithsonian Institution—1846
Dr. Charles G. Abbot

Institution of Mechanical Engineers—1847
Mr. Loughan St. L. Pendred

Real Academie de Ciencias de Madrid—1847
Professor Henry A. Pilsbry

Akademie der Wissenschaften in Wien—1847
Dr. Albrecht Penck

American Association for Advancement of Science—1848
Professor Arthur A. Noyes

Royal Meteorological Society—1850
Mr. Alexander McAdie

American Geographical Society—1852
Dr. Isaiah Bowman

American Society of Civil Engineers—1852
Mr. George S. Webster
Mr. Richard L. Humphrey

California Academy of Sciences—1853
Dr. Charles G. Abbot

K. Academie van Wetenschappen—1855
Professor C. van Vollenhaven

Wagner Free Institute of Science—1855
Mr. Samuel T. Wagner

Institution of Engineers and Shipbuilders, Scotland—1857
Mr. Kenneth Macalpine

University of Chicago—1857
Professor Charles H. Maxson

Numismatic and Antiquarian Society—1858
Mr. J. Henry Scattergood
Mr. David Milne

Société d'Anthropologie de Paris—1859
Professor George G. MacCurdy
Professor Ales Hrdlicka

Massachusetts Institute of Technology—1861
Mr. Waldemar Lindgren

National Academy of Sciences—1863
President Albert A. Michelson

Torrey Botanical Club—1865
Dr. D. T. MacDougal

Cornell University—1865
Dr. Livingston Farrand

Davenport Public Museum, formerly Davenport Academy —1867

University of Illinois—1867
Professor William Trelease

American Museum of Natural History—1869
Professor Henry F. Osborn

American Philological Association—1869
Professor William A. Heidel

Berliner Anthropologische Gesellschaft—1869

University of Cincinnati—1870
Dr. Louis T. More

Wisconsin Academy of Sciences, Arts and Letters—1870
Dr. Charles E. Allen

Institution of Electrical Engineers, London—1871
Mr. Gano Dunn

Philosophical Society of Washington—1871
Dr. L. A. Bauer
Dr. F. E. Wright
Dr. T. D. Cope

Royal Anthropological Institute of London—1871
Professor J. L. Myres
Dr. Ales Hrdlicka

Asiatic Society of Japan—1872
Mr. Roland S. Morris
Mr. B. S. Lyman

The Physical Society of London—1874
Professor H. A. Wilson
Professor Wm. F. G. Swann

Société Geologique de Belgique—1874
Mr. David White

Ecole d'Anthropologie—1875
Mr. J. J. Haffner

Johns Hopkins University—1876
Professor Herbert S. Jennings
Professor Edward Berry

American Chemical Society—1876
Professor Marston T. Bogert
Dr. Irving Langmuir

Archæological Institute of America—1879
Professor Edward H. Heffner

The Engineers Club—1879
President Clayton W. Pike
Vice-President Benjamin Franklin
Secretary Charles E. Billin

Biological Society of Washington—1880
Dr. H. T. Jackson
Dr. T. S. Palmer

American Society of Mechanical Engineers—1880
Dr. Ambrose Swasey
Dr. Calvin W. Rice
Mr. William Elmer
Mr. Conrad Lauer

Société Francaise des Electriciens—1883
Professor Arthur A. Kennelly
Mr. Charles J. Russell

American Institution of Electrical Engineers—1884
Mr. H. P. Liversidge

American Historical Society—(Associated 1884)
Dr. Henry O. Taylor

Modern Language Association of America—1884
Professor Carrol Marden

Sociedad Cientifica Antonia Alzate Mexico—1884
Professor Charles Harrison

Naval War College—1884
Commander Hugo W. Koehler

Bryn Mawr College—1885
President Marion Edward Park

American Economic Association—1885
Professor Emory R. Johnson
Professor E. M. Patterson

Royal Geographical Society Australasia, Queensland—1885
Dr. James P. Thomson

Bureau of Biological Survey—1885
Dr. W. B. Bell

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National Geographic Society—1888
President Gilbert Grosvenor
Mr. Frederick V. Coville

American Mathematical Society—1888
Professor H. B. Fine
Professor I. E. Dickson
Professor G. D. Birkhoff

Temple University—1888
President Charles E. Beury
Dean James H. Dunham

Geological Society of America—1888
President Arthur Keith
Dr. Florence Bascom

American Academy of Political and Social Sciences—1889
Dr. L. S. Rowe
Dr. Carl Kelsey
Dr. J. P. Lichtenberger

Missouri Botanical Garden—1889
Dr. George T. Moore

Geographical Society of Philadelphia—1893
Mr. Alfred M. Collins

Carnegie Museum of Pittsburgh—1896
Dr. William J. Holland

American Physical Society—1899
Professor E. P. Adams
Professor W. F. Magie

Carnegie Institution of Washington—1902
President John C. Merriam
Dr. Charles B. Davenport

Department of Commerce—1903
Dr. George K. Burgess

American Political Science Association—1904
Professor Charles G. Fenwick

The Dropsie College—1907
Professor Max L. Margolis

American Phytopathological Society—1909
Dr. Harold Peirce
Dr. F. V. Rand

Rice Institute—1912
President Edgar O. Lovett
Professor Harold A. Wilson

Universität Frankfurt—1914

National Research Council—1916
Mr. Gano Dunn
Mr. Vernon Kellogg

Laura Spelman Rockefeller Memorial—1918
Mr. Meyrick H. Carré
Mr. David Williams

American Council of Learned Societies—1920
Professor James A. Montgomery

History of Science Society—1924
Dr. Edgar Fahs Smith

Society of Forestry in Finland—1909
Dr. John W. Harshberger

Botanischer Garten und Botanisches Museum, Berlin

MINUTES

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MINUTES.

Stated Meeting of the Society, January 7, 1927.

FRANCIS X. DERCUM, A.M., M.D., Ph.D., Vice-President, in the Chair.

The decease was announced of the following members:

James Wilson Bright, Ph.D., Litt.D., November 29, 1926, at Baltimore, Md., æt., 74.

Henry Algernon duPont, December 31, 1926, at Winterthur, Delaware, æt., 88.

Paul Haupt, M.A., Ph.D., LL.D., December 26, 1926, at Baltimore, Md., æt., 68.

Dr. Zelia Nuttall read a paper on the "The Temple of the Water-goddess at Teotihuacan, a Toltec Historical Monument," illustrated by lantern slides.

After the paper Mrs. Nuttall exhibited by lantern slides photographs of the Statues, Bas-Reliefs and Busts of North American Indians in the Pontifical Museum of the Lateran. These statues were executed by Ferdinand Petrick a pupil of Thorwaldsen and shown for the first time in America, by Mrs. Nuttall at this meeting.

The following papers were read by title:

"An Interesting Behavior of Soluble Meta-Tungstates," by Edgar F. Smith.

"Behavior of Organic Bases toward Soluble Tungstates," by Edgar F. Smith.

"The Fluogermanates of the Univalent Metals," by John Hughes Müller.

The dates for the General Meeting and the Bicentenary Celebration viz., April 27, 28, 29 and 30, 1927, were formally ratified.

Stated Meeting of the Society, February 4, 1927.

FRANCIS X. DERCUM, A.M., M.D., Ph.D., Vice-President, in the Chair.

The decease was announced of the following members:

Arnold E. Ortmann, Ph.D., Sc.D., January 3, 1927, at Pittsburgh, Pa., æt., 63.

Simeon E. Baldwin, A.M., LL.D., January 30, 1927, at New Haven, Conn., æt., 87.

George B. Gordon, Sc.D., January 30, 1927, at Philadelphia, Pa., æt., 57.

James F. Rhodes, LL.D., D.Litt.

Dr. Victor G. Heiser read a paper on the "Health Work of the League of Nations." The paper was discussed by Dr. Abbott and Dr. Keen.

Professor Monroe B. Snyder presented an informal communication on Recently Discovered Proofs of the Validity of the Atomic Numbering of his Fundamental Periodic Table of the Chemical Elements.

Stated Meeting of the Society, March 4, 1927.

FRANCIS X. DERCUM, A.M., M.D., Ph.D., Vice-President, in the Chair.

The decease was announced of the following members:

Charles D. Walcott, Sc.D., Ph.D., LL.D., February 9, 1927, at Washington, D. C., æt. 76.

Sydney George Fisher, B.A., Litt.D., LL.D., February 22, 1927, at Philadelphia, Pa., æt. 70.

Mr. Price moved the following resolution which was adopted by the Society:

Resolved that, until the further order of the Society, the Treasurer be directed at the close of each year to transfer to the principal of each Trust Fund any balance of income of such Fund remaining unexpended at that date.

Bicentenary Celebration and Stated General Meeting, April 28, 29, 30, 1927.

Executive Session.

FRANCIS X. DERCUM, A.M., M.D., Ph.D., Vice-President, in the Chair.

Alfred M. Whitehead and Whitman Cross, recently elected members subscribed the laws and were admitted into the Society.

The decease was announced of the following members:

Edwin Swift Balch, A.B., March 15, 1927, at Philadelphia, Pa., æt. 71.

William H. Dall, A.M., Sc.D., LL.D., March 27, 1927, at Washington, D. C., æt. 82.

Charles Sprague Sargent, A.B., LL.D., March 22, 1927, at Jamaica Plain, Mass., æt. 86.

Erwin F. Smith, B.S., Sc.D., LL.D., April 6, 1927, at Washington, D. C., æt. 73.

Carl H. Eigenmann, A.M., Ph.D., April 24, 1927, at San Diego, Calif., æt. 64.

Morning Session.

FRANCIS X. DERCUM, A.M., M.D., Ph.D., Vice-President, in the Chair.

The delegates were marshalled at 10 o'clock by Dr. James A. Montgomery and at 10.30 were introduced to the President in the order of the seniority of the Institutions they represented and each presented his credentials.

The following papers were read:

"The Origin and Activities of the American Philosophical Society" and an Address on the Dynamic Factor in Evolution, by Francis X. Dercum, Emeritus Professor of Neurology, Jefferson Medical College.

"The Endocrine Organs as a Foundation for Scientific

Medicine," by Charles E. Sajous, Professor of Applied Endocrinology, University of Pennsylvania, Graduate School of Medicine, Philadelphia.

"The Development of Biological Research in America," by Clarence E. McClung, Professor of Zoölogy, University of Pennsylvania. (By request.)

"Roger Bacon," by Dean R. B. Burke, Professor of Latin, University of Pennsylvania. (Introduced by Walton Brooks McDaniel.)

"The Acoustic Impedance of Straight Cylindrical Air Columns in Brass Tubes," by Arthur E. Kennelly, Professor of Electrical Engineering, Harvard University, and R. P. Siskind. (Read by title.)

Thursday Afternoon.

Executive Session.

FRANCIS X. DERCUM, A.M., M.D., Ph.D., Vice-President, in the Chair.

The Society proceeded to an election of Officers and Members.

The Tellers subsequently reported that the following officers and members had been duly elected:

President.

Francis X. Dercum.

Vice-Presidents.

Henry F. Osborn,
William W. Campbell,
James H. Breasted.

Secretaries

Arthur W. Goodspeed,
John A. Miller.

Curator
William P. Wilson.

Treasurer.
Eli Kirk Price.

Councillors.
(To serve for three years.)
Edwin Grant Conklin,
Cyrus Adler,
William C. Sproul,
Charles F. Brush.

Members.
Roy Chapman Andrews,
Arthur Powell Davis,
Joseph Erlanger,
Irving Fisher,
Paul Darwin Foote,
Tenney Frank,
Frank Pierrepont Graves,
Robert Henderson.
B Smith Hopkins,
Max Leopold Margolis,
Gerrit Smith Miller, Jr.,
Thomas Lynch Montgomery,
William Lyon Phelps,
J(acob) Parsons Schaeffer,
Albert Barnes Weimer.

Afternoon Session, 2 o'clock.

FRANCIS X. DERCUM, A.M., M.D., Ph.D., President,
in the Chair.

The following papers were read:

"The Settlement of Tropical South America," by Adolpho Lutz, Research Biologist, Oswaldo Cruz Institute, Rio de Janeiro, Brazil. (By request.)

"Platonism and the History of Science," by Paul Shorey, Professor of Greek, University of Chicago. (By request.)

"Cosmopolitanism in German Romantic Thought," by Kuno Francke, Professor Emeritus of the History of German Culture, Harvard University. (Read by Professor Cheyney.)

"Idiom of Jutland," by John Dyneley Prince, Professor of Slavonic Languages, Columbia University. (Read by title.)

Friday Morning, April 29th,

10 o'clock.

ALBERT P. BRUBAKER, A.M., M.D., LL.D., in the Chair.

The following papers were read:

"Is the Atomic Number of Uranium 92 or 96?" by Professor Monroe B. Snyder, Director Emeritus of the Philadelphia Observatory.

"The Greenland Expedition of 1927 of the University of Michigan," by William H. Hobbs, Professor of Geology, University of Michigan.

"Accomplishments of Modern Astronomy," by Charles G. Abbot, Director, of the Astro-Physical Observatory, Smithsonian Institution. (By request.)

"Persistent Generation of Heat in some Rocks and Minerals, and its Probable Significance," by Charles F. Brush, of Cleveland.

"Recent Developments in Spectroscopy," by Robert A. Millikan, Director of the Norman Bridge Laboratory of Physics, California Institute of Technology, Pasadena. (By request.)

"The Widmanstättian Structure of Iron Alloys," by Albert Sauveur, Professor of Metallurgy, Harvard University. (Read by title.)

"Dynamical Theory of the Rotations of the Planets and

of the Direct Retrograde Motions of their Satellites, with Especial Reference to the Untenable Doctrine of Planetary Inversion," by T. J. J. See, Professor of Mathematics, Naval Observatory, Mare Island, California. (Read by title.)

"The First Description of the North Pole," by Edwin Swift Balch. (Read by title.)

Friday Afternoon, 2 o'clock.

HENRY H. DONALDSON, A.B., Ph.D., Sc.D., in the Chair.

The following papers were read:

"Valence," by W. A. Noyes, Professor of Chemistry, University of Illinois. (By request.)

"Photoelectric Photo-engraving," by Herbert E. Ives, Bell Telephone Laboratories, New York City.

"The Development of Botany in the United States," by John M. Coulter, Boyce Thompson Institute, for Plant Research, Yonkers, New York. (By request.)

"The Presence and Effects of Lipoids in Plant Cells," by Daniel T. MacDougal, Director of the Laboratory for Plant Physiology Carnegie Institution of Washington and Dr. Vamoraveh of Czechoslovakia. Discussed by Dr. Sajous.

"Lamarck's Evening Primrose (*Oenothera Lamarckiana* Seringe) was a Form of *Oenothera grandiflora* Solander," by Bradley Moore Davis, Professor of Botany, University of Michigan.

"The New Tropical Station of the Missouri Botanical Garden at Balboa, Canal Zone," by George T. Moore, Professor of Botany, Washington University, Director of the Missouri Botanical Garden.

"The Organic Content of the Water in Small Lakes," by Edward A. Birge, President Emeritus, University of Wisconsin, and Chauncy Juday. (Read by title.)

Friday Evening Lecture.

Henry F. Osborn, President American Museum of Natural History, spoke on "Recent Discoveries Relating to the Origin and Antiquity of Man." (Illustrated.)

Saturday Morning, April 30th,

10 o'clock.

HENRY FAIRFIELD OSBORN, Sc.D., Ph.D., LL.D.,
Vice-President, in the Chair.

The following papers were read:

"The Skull of Eusmilus," by William J. Sinclair, Associate Professor and Curator, Princeton University, and G. S. Jepsen. Discussed by Professor Scott.

"Development of American Palæontology," by William B. Scott, Professor of Geology, Princeton University. (By request.)

"Age Changes in Anthropometrical Characters during Childhood and Adult Life," by Clark Wissler, Professor of Anthropology, Yale University; Curator of the American Museum of Natural History, Discussed by Dr. Osborn and Dr. Hrdlicka.

"The Origin of Man from the Anthropoid Stem—When and Where?" by William K. Gregory, Professor of Palæontology, Columbia University.

"Is there Proof of the Existence of Pre-Glacial Tertiary Man in America?" by Henry F. Osborn, President of the American Museum of Natural History.

"Changes in Mean Sea Level as an Evidence of Coastal Subsidence," by Douglas Johnson, Professor of Physiography, Columbia University.

"Metabolic Changes in the Body of Female Pigeons at Ovulation," by Oscar Riddle, Carnegie Institution, Cold Spring Harbor, New York.

"Gaseous Metabolism of Wild Birds in Aviary Life," by

Francis G. Benedict, Director of the Nutrition Laboratory, Carnegie Institution of Washington, and Edward L. Fox. Discussed by Dr. Lusk and Dr. Stockard.

"The Genesis of Alkaline Rocks," by C. H. Smyth, Jr., Professor of Geology, Princeton University. (Read by title.)

Saturday Afternoon, 2 o'clock.

FRANCIS X. DERCUM, A.M., M.D., Ph.D., Vice-President, in the Chair.

Albrecht F. K. Penck, A. V. Williams Jackson, Harold A. Wilson, Henry O. Taylor, Charles Stockard and Gano Dunn subscribed the laws and were admitted into the Society.

The following papers were read:

"How History Can be Made a Science," by Edward P. Cheyney, Professor of History, University of Pennsylvania.

"The Growth of Anglo-American Law since the Days of the Junto," by Hampton L. Carson, President of the Historical Society of Pennsylvania. (By request.)

"Geography among the Earth Sciences," by Albrecht F. K. Penck, Professor of Geography, University of Berlin. (By request.)

"University Education in the United States," by James Roland Angell, President of Yale University. (By request.)

"Democracy and the Control of Foreign Affairs," by William E. Lingelbach, Professor of Modern European History, University of Pennsylvania. (By request.)

Stated Meeting of the Society,

November 4, 1927.

FRANCIS X. DERCUM, M.D., Ph.D., Sc.D., President, in the Chair.

J. Parsons Schaeffer, Albert Barnes Weimer and Thomas L. Montgomery newly elected members subscribed the laws and were admitted into the Society.

The decease was announced of the following members:

Ira Remsen, A.B., M.D., Ph.D., March 4, 1927, at Baltimore, Md., æt. 81.

William Powell Wilson, B.S., Sc.D., May 12, 1927, at Philadelphia, Pa., æt. 82.

Joseph Swain, M.S., LL.D., May 19, 1927, at Swarthmore, Pa., æt. 70,

Thomas Willing Balch, A.B., L.H.D., June 7, 1927, at Philadelphia, Pa., æt. 59.

Charles Frederic Mabery, Sc.D., June 26, 1927, at Portland, Me., æt. 77.

Gustav von Tschermak, July 15, 1927, at Vienna, æt. 91.

Edward Bradford Titchner, Sc.D., Ph.D., LL.D., Litt.D., August 3, 1927, at New York City, æt. 60.

Bertram Borden Boltwood, Ph.D., August 16, 1927, at Hancock Point, Me., æt. 57.

William Libbey, M.A., Sc.D., September 6, 1927, at Princeton, N. J., æt. 72.

Henry Larcom Abbot, LL.D., October 2, 1927, at Cambridge, Mass., æt. 95.

Svante August Arrhenius, Dr. Phil. Juris. et Med., October 3, 1927, at Stockholm, Sweden æt. 68.

George Vaux, Jr., S.B., LL.D., October 24, 1927, at Bryn Mawr, Pa., æt. 64.

Antonio Penafiel.

Giovanni Capellini.

Gulielmo Mengarini, at Rome.

John C. Merriam, President of the Carnegie Institution of Washington read a paper on "Science and Appreciation of Nature."

The paper was discussed by Mr. Carson, Dr. Scott and Dr. Donaldson.

Secretary Goodspeed announced that owing to the death of William P. Wilson on May 12, 1927, Curator of the American Philosophical Society since 1902, the office of Curator is vacant, and moved the nomination of Albert P.

Brubaker to fill the vacancy. This nomination was seconded by Professor Scott.

Thomas B. Osborne's resignation as a Councillor was accepted with regret and Hampton L. Carson was nominated to fill this vacancy.

Stated Meeting of the Society,

December 2, 1927.

FRANCIS X. DERCUM, M.D., Ph.D., Sc.D., President,
in the Chair.

Albert P. Brubaker was unanimously elected Curator to succeed William P. Wilson and Hampton L. Carson as a member of the Council to fill the vacancy caused by the resignation of Thomas B. Osborne.

The minutes of the Stated Meeting of the Council held on November 18th were submitted and approved.

ADDENDUM

Please insert the following in your copy of the PROCEEDINGS, Vol. LXVI, 1927, Minutes, page xiii, before the last paragraph.

Dr. Daniel T. MacDougal, Director of Plant Physiology, Carnegie Institution of Washington, read a paper on "Substances Regulating the Passage of Materials into and out of Plant Cells; the Lipoids." The paper was discussed by Drs. Sajous, Donaldson, Brubaker and Harshberger.

REPORT OF
THE PROCEEDINGS

THE ORIGIN AND ACTIVITIES OF THE AMERICAN PHILOSOPHICAL SOCIETY AND AN ADDRESS ON THE DYNAMIC FACTOR IN EVOLUTION

By FRANCIS X. DERCUM

(*Read April 28, 1927*)

I. The Origin and Activities of the American Philosophical Society

Ladies and Gentlemen:

At the General Meeting of the American Philosophical Society held in 1925, there was passed a resolution on April 25th, that the Society should celebrate the Two Hundredth Anniversary of its Foundation at the General Meeting to be held in April, 1927; the wording of the resolution is as follows: "That preparations be made to invest the annual meeting of the Society in 1927 with a dignity and public importance worthy of so notable an anniversary in the world's history of learning." The president, Dr. Charles D. Walcott, then appointed a bicentenary committee. In accordance with the action of this committee, learned societies and universities, both at home and abroad, were invited to participate by sending representatives. The bicentenary committee and the committee on the annual general meeting jointly and in due course arranged the programme.

The Two Hundredth Anniversary of the Foundation of the American Philosophical Society, the oldest scientific body on the Western Hemisphere, gives pause for serious thought and reflection. A careful study of all the available evidence shows that the American Philosophical Society had its origin in the Junto formed by Benjamin Franklin in 1727. Attention has recently been directed to this historical event, with the presentation of numerous important data, in the beautiful and

illuminating address read in commemoration of the late Dr. I. Minis Hays before the Philosophical Society on February 5, 1926, by the Hon. Hampton L. Carson. The fact, however, that so recently as 1906, at the celebration of the bicentenary of Franklin's birth, the date of the founding of the Society was placed in the year 1743 and that this date appeared upon the invitations, programme and other printed matter connected with the celebration, necessitates at this time a brief statement as to the events which led to the definite establishment of 1727 as the date of origin.

Although the story of the Junto has been told many times, the facts have an appealing and fascinating interest. Franklin was at the time only twenty-one years of age and had arrived in Philadelphia only four short years before; eighteen months of this time he had been abroad in London, and yet he had gathered about him a group of young men kindred in spirit, to whom he communicated his own ideals and aspirations. It is not necessary here to enumerate the original members; there were ten at first beside himself; the number was limited to twelve. This club, as Franklin himself called it, was established for mutual improvement. The subjects of their discussions embraced natural philosophy, morals and politics, and were alike interesting and fruitful. Nothing in Franklin's entire career is more significant than this founding of the Junto; nothing is more expressive of the breadth of his rapidly expanding mental vision; nothing more expressive of his amazing personality. Indeed, his forming of the Junto at the early age of twenty-one was prophetic of the great future that was to be his.

From such evidence as we can gather, this Junto—this union of young men for mutual improvement—underwent various fluctuations in its activity. It is not ungenerous perhaps to say, nor is it a wide departure from the truth to say, that the periods of relative inactivity corresponded more or less with Franklin's absences from Philadelphia or with periods in which he was heavily burdened with a multitude of other affairs. It appears that as other matters of business

pressed upon the members of the Junto, and with the advance of time, these periods of inactivity finally became very prolonged, so that the organization at times was practically dormant. It is then that in 1750 we learn of a Junto which contained, it would seem, none of the older members, but was made up of young men, three of them being sons of the founders. It would appear that this was an effort at resuscitating or reviving the old Junto, or, as some have thought, of establishing a new Junto based, or at least modeled, upon the old.

In 1760, the Junto again suffered from a period of inactivity but was finally again revived in 1761, the rules for admission, that is, the qualifications for membership, being the same as in the original Junto and couched in nearly—but not exactly—the same words; as, through the early records having been lost, the qualifications were reproduced from memory. On December 13, 1766, the name of the Junto was changed to the American Society for Promoting and Propagating Useful Knowledge Held in Philadelphia. The importance and significance of this change at once becomes apparent. Speaking of the Junto and of the American Society, a venerable and revered former President of the American Philosophical Society, Mr. Peter Stephen Du Ponceau,[1] expressed himself in 1840 as follows: "The one contemplates an association of students, as it were, for their mutual improvement and the rules to which it is prefixed correspond with that title, the other aims at much greater objects, 'the promotion' and if that were not enough, the 'propagation' of knowledge. Here, then, is a learned Society at once formed; the design is manifest, the foundation is laid and a suitable superstructure only is wanting. The Committee who reported the rules did not dare to wander out of the road traced by Franklin but there was a man of genius, Charles Thomson, whose views extended further, who did not communicate them at once, in all their extent, but gradually led his associates into the path of which his clear mind perceived the honorable end.

[1] "An Historical Account of the Origin and Formation of the American Philosophical Society," p. 26.

"This memorable innovation took place in December, 1766. The next step was to extend the list of the associates. We see the names of thirty members subscribed to the amended Rules, several of whom, but we do not know how many, came in during the following year. In the proceedings of that year we see much talk about enlarging the Society, that is to say, extending its views and its objects; but nothing effectual was done until the year 1768, when things were brought to a conclusion, which was, I believe, very little expected, at least by those who were not in the secret."

Another event of great importance must now be called to mind. Quite a number of years before the formation of the American Society, namely, in 1743, Franklin had issued a "Proposal for promoting useful knowledge among the British plantations in America."[1] His object was to establish a "Philosophical Society." His proposal was in the form of a circular letter which the author placed before his different correspondents. In that letter, after noticing the vast extent of the country, its various productions, and the improvements, manufactures, etc., of which it was susceptible, he observes "that the first drudgery of settling new colonies was then pretty well over, and there were many in every province, in circumstances that set them at ease, and afforded them license to cultivate the finer arts, and improve the common stock of knowledge." To such of those as were men of speculation, he addressed his plan which was: "That one Society should be formed of *virtuosi* or ingenious men, residing in the several colonies, to be established in the City of Philadelphia, as the most central place, and to be called 'The American Philosophical Society.'" He offered to be its Secretary until, he modestly said, "a better one could be provided."

Let me quote again from Mr. Du Ponceau's account:[2]

"The hopes of Dr. Franklin were not fulfilled. It is probable that he was not seconded. No records of that Society, if they had any, have been preserved prior to 1768.

[1] *Loc. cit.*, p. 28.

[2] *Loc. cit.*, p. 31.

In the list of their members, which they gave to the American Society in December of that year, under the head of 'Old Members under the Original Plan begun 1743,' we find only the names of B. Franklin, William Coleman, John Bartram, Thomas Bond, Phineas Bond, Samuel Rhoads, and Francis Alison, seven in all; and all, except the last, who was the Vice-Provost of the College, part of the nine mentioned in Franklin's letter above cited. Among the corresponding members, there is only the name of Cadwallader Colden, elected in 1743, all the rest in 1768.

"Three new resident members, however, the two Doctors Shippen and Mr. John Lukens, appear to have been elected in November, 1767; another, Dr. Cadwallader Evans, was also elected, but declined.

"This is all we know of the 'Philosophical Society' prior to 1767.

"This Society appears at no time to have been very active and it was only in 1767 that a serious attempt was made to revive it. The minutes of the revived Society begin with January 19th, 1768. It at once became active and made preparations for the observations of the transit of Venus which was expected on the 3d of June in the following year. It was also proposed to effect a union with the American Society held at Philadelphia for promoting and propagating useful knowledge, whose views had been nearly the same with those which are published in the American Philosophical Society, and it being judged (by this Society) that the ends proposed by both could be carried on with more advantage to the public, if a union could take place between them, it is proposed that such a union may take place." [1] The Philosophical Society elected in the month of February, 1768, the members of the American Society by their names and sent them notice of their election. This action was rejected by the American Society, who on November 4th, of the same year, elected Benjamin Franklin as their President, although the latter was at that time in England. On November 15th, the following appears on the minutes of the Philosophical Society:

[1] *Loc. cit.*, p. 37.

"This Society having been informed that there is a disposition in the members of the American Society for promoting useful knowledge to unite with us, and that they would appoint a Committee to treat with a Committee of this Society upon the subject, and we, being desirous to promote such a union, do appoint Dr. Shippen, Dr. Bond, Mr. Syng, Mr. Rhoads, Dr. Smith and Mr. Ewing to be a Committee to concert measures and prepare the way for such union, and to make report of their proceedings at the next meeting." [1]

"On the 18th the Secretary of the American Society made known the above to that body, who considering that the views of both Societies were the same, and being persuaded that the ends proposed by both might be better answered and with more advantage to the public if a union should take place, appointed their Vice-President, the two Secretaries, with Messrs. Morgan, Nicola, and Paschall to be their Committee of Conference, to whom they gave special instructions as to the terms to be agreed upon." [2]

The committees met, worked harmoniously, a formal union was established on December 20th, and a new name for the united body was agreed upon; namely, the American Philosophical Society, held at Philadelphia for promoting useful knowledge, the name which our Society has borne since that date. The first election of the officers of the United Society was held on the 2d of January, 1769.

Many years subsequently the question was raised as to the date of the origin of the American Philosophical Society. In 1840, the venerable and reverend President of the Society, Mr. Peter Stephen Du Ponceau, who had been a member since 1791, read at an adjourned meeting held June 26th, a very full historical account of the origin and formation of the Society and he traced our beginning to the founding of the Junto by Franklin in 1727. His conclusion, however, did not meet with unequivocal acceptance. Mr. J. Francis Fisher sent a communication to the Society which was read on the same evening as Mr. Du Ponceau's paper. In it, though most

[1] *Loc. cit.*, p. 43.
[2] *Loc. cit.*, p. 44.

courteously, Mr. Fisher differs from Mr. Du Ponceau and fixes the date as 1743, the year in which Franklin established, though ineffectually, the Philosophical Society. The main reason for his contention was his inability to accept the Junto of 1750 as really a continuation of the Junto of 1727. The question was finally referred to a committee, consisting of Franklin Bache, J. K. Kane, A. D. Bache and Robley Dunglison, who presented a report on October 15, 1841, substantially in agreement with the position assumed by J. Francis Fisher; to wit:[1]

"That the evidence before the Committee does not establish the identity of the Junto which was formed by Franklin in 1727, with that which afterwards became the American Society, though they appear to have been the same in many marked particulars." Further:

"That dating from the establishment of the elder parent-society our centennial anniversary should be celebrated on the 14th (25th new style) of May, 1843."[2] Thus the matter rested until comparatively recently. At the time of the celebration by this Society of the Bicentenary of Franklin's birth, the date of the Founding of the American Philosophical Society placed upon the cards of invitation and upon other printed matter connected with the celebration, was, as already stated, 1743. However, so prevalent was the belief that the Society really had its origin in the Junto of Franklin founded in 1727, that four years subsequent to the celebration of the Bicentenary of Franklin's birth, namely in 1910, the question was again taken up and referred to a special committee. This committee was appointed February 18, 1910, and made its report on May 1, 1914. It consisted of the following members: Hon. Charlemagne Tower, Hon. James T. Mitchell, Hon. Mayer Sulzberger, Mr. Samuel Dickson, Hon. Hampton L. Carson, Professor Edward P. Cheyney, Mr. Francis B. Gummere, Dr. William W. Keen and Dr. I. Minis Hays.

It need not be said that the whole subject was carefully

[1] *Loc. cit.*, p. 97, paragraph 4.
[2] *Loc. cit.*, p. 165.

reviewed. Among other things this committee pointed out that the report of the older committee (*i.e.*, the committee of 1841), apart from its recommendation in regard to the centennial anniversary, was designedly inconclusive. "It must be admitted,"[1] says this older committee, "that chasms still remain in our early annals which require to be filled up, that doubts exist upon some points and discrepancies of opinion on others." In the hope that more facts might be obtained to fill these gaps, they recommended that neither Mr. Du Ponceau's paper nor Mr. Fisher's be published, and that "both be deposited in the archives as valuable contributions to the early history of the Society." "It is clear," says the committee of 1910, "that the committee (of 1840) did not regard the year 1743 as absolutely fixed for the official date of the foundation of the Society; they simply recommended it as the terminus from which to reckon their centenary, as the date of 'the establishment of the elder parent-society.' There was no question in regard to that date; and, as 1827 was long passed, the approaching year 1843 seemed to the committee to be clearly indicated for the celebration. Their action, therefore, did not finally dispose of the question whether or not the origin of the American Society is to be traced to the Junto founded by Franklin in 1727; and it is this question which your present Committee has considered. We have carefully reviewed the evidence presented to the Committee of 1840, which is now printed and accompanies this report; we have given due attention to such new evidence as could be found; and we have been guided by the same principle that was in the minds, though perhaps not quite enough in the view, of the older Committee, which has been formulated in the ruling of the Carnegie Foundation for the case of educational institutions, to wit, that 'by date of founding is meant the year in which the institution was established, out of which the present college or university has developed.'" After a further consideration of all the available data, the committee of 1910, concludes with the statement[2] "Nothing

[1] *Loc. cit.*, p. 166.
[2] *Loc. cit.*, p. 185.

had been alleged or discovered which seems adequate to offset that testimony and that tradition." (Namely, that the Society had its origin in the Junto of 1727.) "There is now (continues the committee) no centennial celebration to be justified; the zeal, the partisan sharpness of controversy, which was controlled, it is true, by the fine courtesy of those gentlemen whom we are proud to call our predecessors, but which, nevertheless, can be read between the lines of their report, is now stilled; and your Committee is constrained to the opinion that 1727 was the date of the foundation of this Society and it is so reported." The report is signed by all of the members of the committee save Professor Cheyney. Professor Cheyney, in the interest of accuracy of historical detail, insists upon the non-identity of the two groups of men constituting the older and the younger Junto.[1] Says Professor Cheyney, "The proof of the non-identity of the two groups of men, both of which refer to themselves as the Junto, does not, it is true, rise to an actual demonstration, but, added to the doubt left from a study of the minutes, it amounts, in my mind at least, to an absolute conviction. With the opposite view on this question taken in the report of the committee, I cannot therefore agree. The two groups of men have very much in common, including the use of the name Junto, but they were two groups of men, nevertheless."

Professor Cheyney, however, begins his separate report with the statement,[2] "I am in entire agreement with the main conclusion of the committee's report, that the origin of the American Philosophical Society should be carried back to the Junto founded by Franklin in 1727; but I am unable to agree with one section of the reasoning by which that result has been reached." He concludes as follows: "If the 'date of founding,' as it has been formulated for the use of colleges by the Carnegie Foundation for the Advancement of Teaching, means 'the year in which the institution was established out of which the present college or university (institution), has

[1] *Loc. cit.*, p. 191.
[2] *Loc. cit.*, p. 187.

developed,' the Junto of 1750 certainly developed out of the Junto of 1727, and the Society may claim its descent through a younger line just as fairly as by primogeniture, so that the date of origin of the Society should be considered that of the formation of Franklin's Junto, in 1727."

A final paragraph is then added to the report of the committee. It reads as follows: "The question whether Franklin was in a proper sense the founder of our Society is not in doubt. He founded the American Philosophical Society in 1743. He founded the Junto of 1727. The development of a junior Junto—if such a thing occurred—was unquestionably due to the impetus of the older Junto; and the change of the local Junto into a Society which included corresponding members from other colonies was a mere broadening of its purpose. In the sense of the ruling of the Carnegie Foundation, it is clear that when Franklin founded his original Junto, he became the founder of the American Society. He is admitted by all to be the founder of the Philosophical Society. Our Society therefore owes its origin to him on both sides." This conclusive statement is signed by every member of the committee and the question of the date of the origin of the American Philosophical Society may be considered as finally and definitely settled and disposed of. There is every justification and propriety therefore in the present celebration of the Two Hundredth Anniversary of our birth.

While the early records of the Junto, of the American and of the Philosophical Societies are very fragmentary and incomplete, there is every indication that in these early years the proceedings were alike interesting and important and laid the broad foundation of the future usefulness and greatness of our Society. Who can doubt, for instance, that in these formative years, Franklin discussed here his own discoveries, and that others also made interesting contributions? It is very gratifying, also, to know that after the union of the two societies, the American and the Philosophical, observations on the transit of Venus were made. These were published in the

first volume of our transactions and attracted the attention of the learned in Europe.[1]

Ever since these early days the Society has been active. Of this the *Transactions* and *Proceedings* furnish abundant proof. A brief reflection calls to mind the names of Jefferson, Rittenhouse, Rush, Rumford, Priestley, Bowditch, Hassler, Fulton, Day, Henry, Asa Gray, the two Agassizs, Langley, Newcomb, Gibbs, Pickering, Leidy, Cope, George Horn, Ryder, Harrison Allen, Jastrow, Walcott. Needless to say a host of other names should be added; and to-day, as in years past, at the annual general meetings of our Society, a galaxy of distinguished scientists bring the choicest results of their latest endeavors.

Upon our walls are the portraits of Franklin, our founder, of Jefferson, the first palæontologist, of Washington, leader of men and apostle of liberty. Upon either side are ranged the portraits of others, each great in the service and in the cause of human progress. All are to-day bearing mute witness to this our Two Hundredth Birthday. "The first four presidents of the Society after the reorganization of 1768 were Franklin, Rittenhouse, Jefferson and Wistar. Fifteen members of the Society were signers of the Declaration of Independence; eighteen members were framers of the Constitution of the United States. Eight members of the Society have become Presidents of the United States [2] and three other Presidents have become members. [3] Twelve members of the Society have been Justices of the Supreme Court of the United States, and of these four were Chief Justices. All seven of the Americans who have received the Nobel Prize were members of the American Philosophical Society.[4] A large proportion of the scientists, publicists and scholars in the United States have

[1] Mr. Du Ponceau's account, *loc. cit.*, p. 41.

[2] George Washington, John Adams, Thomas Jefferson, James Madison, John Quincy Adams, James Buchanan, U. S. Grant, Woodrow Wilson.

[3] Grover Cleveland, Theodore Roosevelt, William H. Taft.

[4] Theodore Roosevelt, 1906; Albert A. Michelson, 1907; Alexis Carrel, 1912; Elihu Root, 1912; Theodore W. Richards, 1914; Woodrow Wilson, 1918; Robert A. Millikan, 1923.

been members of the Society, and the same is true of the more distinguished corresponding foreigners." [1]

One of the distinctive features of this Society is that it has not become divided into sections. All departments of knowledge are presented at the meetings, whether the subject dealt with is general or special in character. All of the members, no matter to what special field of learning they have devoted themselves, no matter in what department their own knowledge may lie, listen at our meetings to subjects most varied in character, all having to do with the advancement of human knowledge. Our ranks include not only scientists and students of the various branches of learning, but also men gifted in the law, gifted in the knowledge of state and government, men of affairs, men who are marching in the forefront of human progress. Truly our Society is in its broadest sense a philosophical society.

II. The Dynamic Factor in Evolution

I have felt that the address of a presiding officer upon so important an occasion as the bicentenary of the Society should be one dealing not exclusively with the general questions arising from the history and activities of the Society, but that he should, if possible, deal with some pregnant question of the day, and I have therefore chosen to present on this occasion what I have ventured to term "The Dynamic Factor in Evolution," and I will ask of you to listen with an indulgent consideration, while I promise on my part to be as brief as lucidity of statement will permit.

The phenomena presented by living things constitute the last stronghold of teleology. Teleology has long ago been abandoned in all other fields of inquiry. The doctrine of design has no application to the truths of physics nor of chemistry, no application to the truths of astronomy nor of mathematics. In these fields the doctrine is automatically cast

[1] Summary by Carroll Frey in "The Independence Square Neighborhood," Philadelphia, MCMXXVI., p. 35. Corrected for the number of Presidents who became members subsequent to their careers as president (3 instead of 2) and also as to the number of the recipients of the Nobel Prize (7 instead of 6).

aside. It is as unnecessary as it is futile. To say a thing is so because it was made so is simply a reversion to a primitive mode of thinking, and because the phenomena presented by living things happen to be more complex is no reason for applying to them an explanation discarded in the other sciences. Such an explanation is a negation of all modes of scientific thinking and is in its essence nothing more than a meaningless restatement. It is alike self-destructive and nugatory.

In the gradual growth of our knowledge, we have developed an attitude toward the physical or so-called inorganic world which the facts prove to be entirely erroneous. Thomson, Rutherford, Millikan, Aston, Mosley, Bohr, Einstein and others have conclusively demonstrated the structure of matter. The atom has resolved itself into its nucleus and revolving electrons, the nucleus or proton being electropositive, the electrons electronegative. The nucleus has further been revealed as being made up both of electropositive and electronegative factors, the electropositive predominating. The conclusion is inevitable that the atom, made up of nucleus and electrons, is only a manifestation of electricity; that is, a manifestation of energy. This interpretation of the structure of matter, and, as a corollary, of the universe, must greatly influence our conceptions regarding living things. The basic distinction between living and non-living substance disappears. Our age-old conceptions regarding non-living matter reveal themselves to be hopelessly at fault. The universe is in no part and at no time "dead" or "inert." Taken in its entirety or in its most minute subdivisions, it reveals itself in its ultimate analysis to be an expression of energy, a moving, pulsating, throbbing, "living" thing, a thing in which we are immersed and of which we form a part. It does not seem philosophical to regard those moving molecular aggregates which we term living forms, no matter how integrated and differentiated they may be, as things separate and distinct from the rest of the universe. Assuredly it does not seem necessary to evoke special agencies to account either for their

existence or for the phenomena which they present. One of the generalizations held out to us by this conception of the universe is that the universe is a whole, all parts of which are expressive of manifestations of energy, manifestations cognate and correlative and intrinsically identical. Of necessity, such a conception must include living matter. If this be so, no other explanation of the phenomena presented by the basic substance of living things, protoplasm, should ever be attempted.

Two possible hypotheses as to the origin of living matter present themselves; first, that living matter is the result of an act of special creation, *i.e.*, of the special intervention of the Deity, and, secondly, that it made its appearance upon the earth as have other substances, as a result of the interaction and combination of previously existing substances and in conformity with natural laws. The first hypothesis may, for the present at least, be set aside. When we approach the second, we meet at first very curiously with another explanation. A number of distinguished thinkers have entertained the hypothesis that life is of extra-terrestrial origin; *i.e.*, that living matter pre-existing in interplanetary space in some way reached and inoculated the earth. Such views were hazarded by Richter, Kelvin, von Helmholtz, Arrhenius and others. Arrhenius, for instance, has expressed the view that life has always existed in space and that living particles propelled by light pressure reached the earth. Notwithstanding the eminence of the authors of such theories, they can only be regarded as futile evasions of the problem.

To my mind, we are forcibly narrowed to the second view, namely, that living matter, as just stated, made its appearance upon the earth as a result of the interaction, combination and association of previously existing substances and in conformity with natural laws. This does not presuppose that it did so suddenly; certainly not in any way that suggests so-called "spontaneous generation;" but as the result of very gradual changes taking place and extending over vast periods of time. This suggests the existence in the remote past of exceedingly

primitive and intermediate substances which in increasing degree developed properties which we have come to associate with living matter. Let us, however, leave this portion of the subject for the present and turn our attention to the living matter known to us to-day.

Living matter, protoplasm, is not a chemical compound. It is known to be an aggregate of many substances; proteins, fats, carbohydrates and crystalloids or electrolytes. The proteins consist of associations or combinations of many amino-acids. The fats and carbohydrates are diffused or suspended in the general protoplasmic mass. The crystalloids or electrolytes are likewise diffused in solution through the general protoplasmic mass and are resolved into their constituent ions. The physical condition of protoplasm is that of a colloid consisting of many aggregates which play the rôles respectively of continuous and discontinuous phases. It is, of course, to the discontinuous phases, many of which appear as separate particles suspended in a liquid, that the granular appearance of protoplasm is due.

Let us pause to emphasize the fact that protoplasm is a colloid. It must therefore obey the laws of colloidal structure. To borrow a phrase from Giulo Fano,[1] the colloidal state is a "dynamic state" of matter. Physically protoplasm is an emulsoid. At once the facts of surface tension and surface energy present themselves. The surface of a liquid is in tension against its own vapor, or, in the case of an emulsoid, against the liquid which constitutes the continuous phase. Obviously, the sum total of surface energy must depend, among other things, on the amount of surface. That the latter is greatly increased in such a structure as a colloid, in which, of necessity, the disperse phase consists of a vast number of minute particles, is readily comprehended. Thus, it is estimated that in a liter of colloidal gold solution of 0.5 gm., in which each particle has a diameter of 15 sub-microns, the total surface of gold is equivalent to 65 square meters. It is clear that analogous conditions exist in protoplasm.

[1] Giulo Fano, "Brain and Heart," translated by Helen Ingleby, Oxford University Press, 1926, p. 7.

The structure of protoplasm is such that an enormous surface is developed. Every particle of protoplasm must therefore be regarded as a vast storehouse of energy. This is the first fact of importance to recognize. We now come to another fact of even greater importance.

It has long been known that certain substances may induce changes in other substances merely by their presence, they themselves undergoing no change whatever. For instance, dilute sulphuric acid added to starch will convert the latter into glucose, the acid itself neither qualitatively nor quantitatively undergoing any change. Again, hydrogen peroxid is decomposed by soluble alkalies, also by the mere presence of insoluble substances such as metallic silver or metallic platinum; these substances as before undergo no change. A similar statement applies to the action of platinum in bringing about the oxidation of alcoholic vapor and to the action of spongy platinum in bringing about the union of hydrogen and oxygen. Similar truths apply to both gold and silver, though neither are as active as platinum. The various substances enumerated, it should be mentioned, may perform the rôle on the one hand of reducing agents or on the other of constructive agents, the chemical bodies resulting being either more simple or more complex in structure. Such action, I need hardly state, is termed catalysis. It implies more especially the setting in motion or the precipitating of possible or impending chemical changes. The action is one in which given substances by their presence alone set into activity chemical processes otherwise dormant, without themselves suffering any chemical change. Some facts are especially suggestive; for instance, in the union of hydrogen and oxygen brought about by the presence of spongy platinum, the enormous increase of surface in the spongy platinum is the immediate causative factor of the union. Such facts as these, so well known, are, I believe, of the utmost importance in interpreting the phenomena of living matter.

It is hardly necessary to call to mind the fact that when a food particle is taken into the interior of an amœba, it gradu-

ally disappears and finally becames part of the substance of the amœba. It would appear that, as in higher animals, the protoplasm of the amœba has the power of fragmenting proteins, fats and carbohydrates. In the higher animals, protein is reduced successively into peptones, amino-acids and a series of intermediate products, until the final stage of urea, uric acid and kindred substances is reached. Carbohydrates are by a long series of intermediate stages converted into carbon dioxid and water. Fats, also, are decomposed into glycerine and fatty acids and by a further reduction into simpler bodies, until end-products are likewise reached. During these changes energy is released. There is no inherent improbability in the inference that changes like or analogous to these take place in the amœba. Certain it is that the various food particles are so metamorphosed that they disappear into the general substance of the organism and can after a time no longer be distinguished. Evidently they have added to the substance of the amœba. Facts and inference both justify the statement that physical increase of size and final separation or division into two bodies follow the continued ingestion of food; and this constitutes the second fact of importance, for it means that the *constructive or upbuilding change far exceeds the reducing change.* That which is observed in the amœba, it is needless to say, is true of other living forms, and, to state it in other and more familiar words, living protoplasm is the seat at one and the same time of both anabolic and catabolic processes, of both an upbuilding and a down building change. The fact that the upbuilding process exceeds the down building process is one of very great significance. Growth and multiplication both have their explanation in this fact. Various terms, it need hardly be added, have come to be applied to the substances instrumental in inducing these changes, such as enzymes, ferments and hormones, but the general term catalysts can be conveniently applied to them all; and the inference further is justified by the facts at our command that their action in no way differs from that of catalysts of the non-living world, for like the latter they act merely by

their presence. How intensive this action may be, is illustrated by the fact that under the influence of enzymes or ferments, chemical changes take place in living organisms which can only be brought about in the laboratory by powerful reagents and high temperatures. For example, the change of protein to amino-acids is affected in the laboratory by boiling in concentrated hydrochloric acid. In the organism it takes place at an equal rate at ordinary temperatures and in a medium which is only just faintly alkaline or neutral.[1]

With these conceptions of catalytic action both in the non-living and the living world, let us turn our thoughts again to the possibilities of the origin of living matter; namely, as the result of very gradual changes taking place and extending over vast periods of time and resulting from the interaction, combination and association of previously existing substances and in conformity with natural laws. In all probability the complex substances that were thus formed consisted at first of exceedingly minute molecular aggregations. These resulted doubtless from the synthesis of less complex bodies. The very fact of such a synthesis implies the early action of catalysis on the part of the intermediate and primitive bodies, bodies which to-day no longer have existence on the earth for reasons to which we shall presently recur. Little by little these aggregates became more and more complex and very gradually and in an increasing degree manifested properties which we to-day associate with living matter. At first these aggregates were quite without definite structure, unless we consider their possible molecular arrangements as structure. They were also without definite form, though it is safe to infer that they were rounded bodies. Little by little and in increasing degree, they manifested properties which finally were frankly those of living matter; namely, constant chemical interchange with the environment, constant increase and constant diminution in substance, all of these phenomena being inextricably interlinked with, and dependent upon the ever increasing catalytic power of the aggregate. The

[1] Bayliss, "Principles of General Physiology," third edition, p. 301, 1926.

living protoplasm of to-day has become the complex aggregate that it is because of the catalytic power of its primitive proteins to force other materials, often of vastly different chemical structure, into association with itself. "Life," let us assume, has thus made its appearance. Let us trace it further.

According to the views formulated by the distinguished bio-chemist of McGill University, Professor A. B. Macallum,[1] life originated in the ocean water of the Archean period and the very earliest organisms must have been of the micellar or ultramicroscopic kind. Later these micellar organisms gave rise to multi-micellar aggregates and still later these gave rise to simple and very undifferentiated cellular forms from which in turn others evolved which were less simple and approximated in complexity the very simplest living organisms of to-day. Further, these organisms synthezised their own constituents from the available nitrogen and carbon dioxid from the air, the sulphur from the sulphates, the phosphorus from the phosphates and the iron from the sea water. There was as yet no nucleus.

This interpretation by Macallum seems to me to bear within it the inherent probability of truth. It is a legitimate inference further, that the synthesis outlined by Macallum resulted in the formation of amino-acids and the association of the latter into groups to form proteins. Probably the formation of proteins occurred long before the cellular stage was reached; indeed, at the time the first molecular aggregations made their appearance.

After the formation of proteins, the next important step was without doubt the differentiation of nucleoproteins and their grouping together to form the nucleus of the cell. According to Macallum, the contents of the normal cell nucleus does not know the inorganic world. "It contains not a trace of potassium, or of chlorides, phosphates, carbonates or sulphates, and accordingly no sodium, calcium or magnesium, although these four elements are found in the cytoplasm."

[1] "Physiological Reviews," Vol. VI., No. 2, April 1926.

When we consider the remarkable rôle which the nucleus plays in the process of cell division, it suggests at once that the nucleus dominates the cytoplasm. In other words, while the cell contents as a whole probably possess catalytic powers in varying degrees, the dominating catalytic power resides in the nucleus. Judging from the facts, it would appear that the ions of the alkaline and earthy bases are automatically excluded from the nuclear material. Possibly to this exclusion is due this dominating catalytic power of the nucleoproteins. It would appear that the presence of crystalloids would tend to inhibit this catalytic power.

A protoplasmic mass with differentiated nuclear material having once made its appearance, a further differentiation next took place. Two protoplasmic aggregates coming into contact or within the range of physical or chemical, or shall we say of catalytic reaction with each other, the mass possessing the greatest catalytic power would dominate the second; *i.e.*, it would cause the second to be so changed by the greater catalytic power of the first as to be capable of being taken into and finally incorporated with the substance of the latter; the latter possessing the more intensive anabolic processes. To state it in physiological terms, the second mass would be digested and assimilated by the first. Herein, I believe, lies the key to the differentiation of living protoplasm into animal and vegetable forms. This "cannibalism," early established, was followed later by a lessened and finally by a loss of the power of the dominant form to construct its proteins and other constituents directly from the materials of the environment. Here then is a forward step of great importance in the progress of evolution. Herein, also, I believe lies the explanation of the disappearance of the intermediate and primitive bodies which preceded the appearance of fully developed protoplasm. In the "catalytic struggle" they failed to survive and under changing conditions have long since ceased to be formed.

Let us return now to another fact already emphasized, and that is that in living protoplasm the building-up or anabolic processes immeasurably exceed the building-down or catabolic

processes. As a result, living protoplasm has spread all over the earth; it has not only invaded sea, plain, valley and mountain, but has penetrated into every corner, nook and cranny of earth, water and air in which the conditions necessary to its interchanges chemical and catalytic exist. All lacunæ are filled. Here is the explanation of its survival and persistence through such vast periods of geologic time. Further, living protoplasm is a plastic aggregate which has been able to adapt itself to the most varied environmental conditions. It certainly does not seem strange that it has assumed the most varied forms. Personally I believe that in the marvellous catalytic power of the protoplasmic aggregate, that power in which the anabolic processes so greatly exceed the catabolic processes, lies the key to the explanation in a large measure of evolution.

Neither time, nor the objects of this address will permit of the application of this dynamic principle to the details of the evolution of animal and vegetable forms. We must content ourselves here with the general conception that further and increasing differentiations are dependent, first upon the materials available in the environment, and, secondly, upon the innate catalytic power of the living protoplasm. Necessarily under given conditions, there ensued an increasing complexity of the chemical changes in the latter. In unicellular forms, as in the amœba, these changes remained relatively simple; not so, however, in the metazoa. Here we find that each cell possesses not only the special structure which enables it to fulfil the functions of the tissue of which it is a part, but also special ferments by means of which it builds itself up, adds to its own substance out of the general material of the blood-plasma. Each cell possesses the power of taking in foreign materials, of fragmenting them, and of utilizing them for purposes of reconstruction or as sources of energy. In return, each cell gives up to the blood-stream such substances as are of no further value to it. These substances, the products of its continued chemical changes, may consist in part of materials so far reduced that they are no longer sources of energy and

are ready for discharge from the organism as waste materials, such as urea; or they may consist of substances which have, first, still a food, or, second, a catalytic value for the cells of other tissues. As an instance of the first may be mentioned the glycogen of the liver; as an instance of the second, the epinephrin of the adrenals.

It is important to envisage continuously the organism as a whole and to bear in mind that all of the tissues of the organism play a rôle in its chemical changes, *i.e.*, in its metabolism. What we have learned of the various glands of internal secretion as to their embryology and their morphology justifies the statement that certain glandular structures—perhaps, like the thyroid, early provided with a duct and serving at first certain ulterior purposes—became the seat of the formation of intensive catalysts serving certain special functions. Certain structures, most diverse in origin and probably serving early in the evolution of the organism other functions, became associated in the general and special metabolism of the organism. Thus the thyroid, the pituitary, the chromaffin system and the sympathetic nervous system became synergically related; they constitute a synergic group. While certain of these, like the thyroid and pituitary, came to influence especially growth and development, the skeleton and other structures, all of them came to act together to promote tissue activity; all of them increase metabolism; all of them promote catabolism and the release of energy. On the other hand certain other structures have to do with the storing-up of energy. The pancreas and, in fact, all of the glands of the alimentary tract together with its adnexa are under the influence of that portion of the sympathetic nervous system spoken of as autonomic. They are all concerned in the processes of digestion and assimilation; in other words, in the storing up of energy. The thymus and parathyroid are both in synergic relation with the pancreas and must, therefore, be added to this group. All are opposed to the disintegration, to the downward change of the body constituents; in other words, to catabolism.

To repeat the facts in other words, the symbiosis of the

cells in metazoa lead gradually to the differentiation of the glands of internal secretion. This meant in an increasing degree, specializations of the catalytic processes of living protoplasm. This implied further in a large measure the specialization of chemical changes in the individual cells, as well as a mutual inhibition of the chemical processes in the organism in its entirety; and this is exactly what occurred.

Almost every one who has thought seriously on the subject of evolution and who has realized the truth, *i.e.*, the fact of evolution, has attempted some explanation of its cause; either framed some explanation to satisfy himself or has sought refuge in the explanations framed by some of the world's great thinkers. Two names, of course, stand forth in striking prominence, that of Lamarck and that of Darwin; the one deals with the rôle which the use of organs may have in their development, the second points out the rôle which nature plays in the struggle for existence and in the survival of the fittest; a fact which has been stated obversely as the death of the unfit. Both explanations lack a primal, driving cause inherent in the living organism itself. Both enter into the explanation of results but not as to origins. Bergson [1] feeling the necessity of a special cause uses the expression the "vital impetus" (élan vital); this, however, merely assumes a fact without offering an explanation as to the origin of the fact; Eldridge uses the term "organizatory factors" [2] which equally fails to explain, while the distinguished Italian physiologist Giulo Fano satisfies himself with the expression the "cosmic will." [3] For myself the term, the "cosmic will"—and I say this with all courtesy—has nothing but the vaguest significance. Like the other terms, it gives us no help in the solution of the problem with which evolution confronts us.

Is the conception of a dynamic factor of any value in the solution of this problem? Of the existence of such a dynamic factor there can be no doubt. The enormous catalytic, con-

[1] Henri Bergson, "Creative Evolution," translated by Arthur Mitchell, Ph.D., 1911.

[2] Eldridge, "The Organization of Life," 1925.

[3] *Loc. cit.*, p. 45.

structive power of living protoplasm is one of nature's outstanding facts. Obviously it must be admitted into our problem. Is it of itself sufficient to explain the differentiations and increasing complexities of organisms? I believe that it is. I believe that, taken in connection with certain other factors which likewise have to do with the reaction of the organism to the environment, it is the one outstanding cause of evolution. The metabolism of the metazoa, complex as this is in the higher forms, cannot be a fixed, and unchanging metabolism. The metabolism of the organism must, of course within certain limits, vary with its environment. The cells of the various tissues of the metazoa have the power of appropriating and assimilating certain materials from the blood plasma, but clearly in the exercise of this function they are dependent upon the materials presented by the blood plasma. The blood plasma in turn must build itself up out of such materials as come to hand and are ingested by the animal. It follows that if given variations in the environment occur, *e.g.*, in the food, these variations must affect the constitution of the blood plasma, and thus the metabolism of the entire organism. From such evidence as presents itself, it is safe to infer that, in the past, variations in the environment, food, character of medium, water or air, temperature and other climatic conditions, took place very slowly and allowed of adaptations of the organism to these gradually changing conditions. It can very readily be comprehended that such a modified metabolism could be transmitted to the offspring, for it would be a general character, one affecting the organism as a whole. Indeed, the question of the heredity of such a modified metabolism need not be considered if we reflect that the young offspring would necessarily be submitted to the same modified environment and would biochemically adapt themselves to it just as had their predecessors; perhaps even more readily because the young are relatively more plastic. With this interpretation in mind, we are able to admit and to dispense with the discontinuity of variations so much insisted upon by Bateson; for the reason that spontaneous variations so-called are really

not a part of the problem. Further, it would appear that increasing complexity and differentiation in response to environmental change can go on almost indefinitely so long as previous adaptations to the environment have not imposed upon the organism such fixations or limitations of structure as to make further adaptations to changing conditions impossible. And yet the existence of such highly modified mammals as the seals and cetaceans, the manatee and the dugong, not to speak of that marvellous aquatic reptile of bygone days, the ichthyosaurus, show how much adaptation is possible even in forms already highly differentiated. Probably we should admit here the play of the theory of Lamarck, bearing in mind that the use or over-use of an organ must in its turn react upon the metabolism of the animal and doubtless in many cases react directly upon given glands of internal secretion. When we reflect again upon the rôle of the pituitary and thyroid in growth and skeletal development, the facts become more than suggestive. Very suggestive too are such facts as the appearance and disappearance of the notochord in the ascidians. It would seem that the to and fro movements of the young in swimming would lead spontaneously to the deposit of gelatinous or other fixative materials in the only portion of the organism relatively at rest and capable of furnishing a point of fixation for muscular action; while in the subsequent sessile condition the notochord thus formed would from non-use again spontaneously disappear. Lamarckism should, I believe, be given its due weight. This is true also, of course, of the great static, inhibiting principle enunciated by Darwin. Each has its rôle to play in our interpretation. Professor Jennings, it is needless to say, lays great stress upon the influence of changed conditions of the environment, and, in his remarkable little book "Prometheus," uses among other things as an illustration the well-known facts of the transition of the aquatic form of axolotl to a terrestrial form.[1]

[1] H. S. Jennings, "Prometheus or Biology and the Advancement of Man," 1925, pp. 45 to 47.

It has seemed to me, further, that in studying the basic problems of both plants and animals, especially of their forms and skeletal structure, another fact should be considered, and that is the rôle which the crystalloids, the electrolytes, play in the results. Crystalloids enter into the composition of the cytoplasm; may they not play a part in the physical form which the organism assumes and in other details of its structure? Certainly the experiments of Leduc, now many years old,[1] and the significance of which, it seems to the writer, has never been sufficiently emphasized, would suggest such to be the case. Leduc, it will be remembered, allowed various mineral salts to crystallize in colloid solutions and obtained structures which many times simulated living forms. While the nucleus of the cell, free of mineral matter, dominates the growth and reproduction of the cell, may it not be that the cytoplasm with its rich content of alkaline and earthy salts plays a rôle in the form, the shell, the skeletal structure of the organism? It would seem that we have to deal, first, with the dynamic constructive influence of the nucleus and, secondly, with the formative or modifying influence of the cytoplasm. The crystalloids, or electrolytes, as perhaps we had best term them, probably act in accordance with the play of their constituent ions. It is astonishing that an adequate and systematic study of the interactions of electrolytes and colloids has never been made.

I have not, in this address, presented a theory of evolution. I have merely called attention to a dynamic, driving principle inherent in living matter. Is this principle not the keynote of that emergent evolution so brilliantly conceived and advocated by Jennings? It leads, first, to an increase in mass and, secondly, to an increasing complexity and differentiation; changes in which the environment necessarily plays a part. Further, the increase in mass leads to a division of the mass, *i.e.*, to multiplication or reproduction. Into this division I believe the same factors enter whether this division be ex-

[1] Stéphane Leduc, "The Mechanism of Life," 1911, translated by W. Deane Butcher.

pressed by simple fission or by the more complex process of mitosis; namely, by the interactions, catalytic and chemical, of the contained colloids and crystalloids. Conjugation and sex differentiation find here also an explanation; an explanation already hinted at in the view presented earlier in this address in explanation of the differentiation of animal and vegetable forms. The problems of heredity would follow a similar course. The conceptions here presented suggest, among other things, the daring thought that the theory of genes is unnecessary. The chromosomes become aggregates of amino-acids or proteins which are the seat of continuous and progressive changes catalytic and chemical. They can only react with the other cell-contents and with each other, in certain definite ways. The changes which take place are necessarily consecutive. They give rise, finally, in the offspring, to certain resultant qualities which are the same or similar to those presented by the parent form. This view seems to me to be preferable to one that assumes the existence in the chromosomes of thousands of minute chemical parcels which contain the necessary materials for the transmission of the various hereditary qualities. In keeping with this thought, Professor Jennings expresesses himself in "Prometheus" as follows: "The characteristics of the adult are no more present in the germ cells than are those of an automobile in the metallic ores out of which it is ultimately manufactured.[1] "

Mendelism, also, presents itself in a new light. Dominant characters, for instance, are those which result from dominant anabolic or constructive processes; the others, the recessive characters are those which are left behind in the "catalytic struggle." A further consideration of other phenomena of heredity, such for instance, as linkage, though they also find here an explanation, would take us too far afield, and I must close with an apology for having trespassed so much upon your time and patience.

[1] *Loc. cit.*, p. 28.

THE ENDOCRINE ORGANS AS A FOUNDATION FOR SCIENTIFIC MEDICINE

By CHARLES E. de M. SAJOUS

(*Read April 28, 1927*)

Although physiologists, ever since the pioneer labors of Claude Bernard and Brown-Séquard, have given us an aggregate of scientific data of inestimable value, the fact remains that they have not been able, so far, to identify the fundamental function of a single endocrine organ. This constitutes an obstacle to medical progress, particularly in the clinical field, which can hardly be overestimated. Over thirty years ago a close analysis of the question as a whole led me to conclude that, owing to the vast field which these organs seemed to cover, no single branch of medicine would ever be able to solve the problems they offer. Conversely, coördination of our knowledge on the subject followed by analysis and synthesis of the aggregate and then applied to the clinical field, would alone, it appeared to me, lead to the truth, and, moreover, point to those features of any given question which required additional experimentation and development. This all-embracing form of investigation has since been termed the "synthesis of sciences" method.

One of the many features disclosed was the presence in our tissues of various substances which, though familiarly known to biochemists and physiologists as constituents of all cells, even of the blood elements, could be connected with no demonstrable function. Among these were certain oxidizing enzymes, lipoids such as lecithin, cephalin, cuorin and cholesterol, and, seemingly out of place, digestive enzymes in all tissue cells. While authoritative works indicated their universal presence in the tissues and their chemical properties, both the rôle they fulfilled and the purpose of the reactions they pro-

duced were either left unexplained or characterized as unknown. All these bodies found a legitimate place, however, when links were revealed by the synthetic method in other directions which connected them with other definitely established biochemical agents in the tissues.

The importance of these various chemical waifs asserted itself when they proved to be normal products of the endocrine organs. Especially did their presence assume paramount proportions when it became evident that they carried on a function which had not been explained since the discovery of oxygen and the labors of Priestley, 1774, and Lavoisier, 1775, that of tissue respiration, through which the process of heat production in our tissues, the life process itself, is sustained. As Professor Howell,[1] of Johns Hopkins, stated in 1924: "the respiratory history of oxygen ceases after this element reaches our tissues." What this means to clinical medicine as a whole may be surmised from the statement of another distinguished physiologist, Professor Halliburton,[2] of Kings College, London, as late as 1921, that "knowledge of tissue respiration is so scanty that we can say but little of its pathological bearing."

This state of things, so disastrous to medicine as a whole, is sustaining the great death rate of pneumonia, pulmonary tuberculosis and many other diseases, while perpetuating the widespread suffering due to various disorders which are not immediately lethal, insanity, for example. Its underlying cause, from my viewpoint, is the uncertain status of the prevailing theory of pulmonary respiration. C. R. Barnes, Professor of Plant Physiology at the University of Chicago, wrote for example, some years ago, after examining the best of our textbooks of physiology: "The respiratory ratio has proved a veritable will-o'-the-wisp, leading investigators into a bog where their labors and their thinking were alike futile. For, as a sign of what is going on within, the respiratory quotient is absolutely valueless." A prominent English physiologist,

[1] Howell, W. H.: *Jour. Amer. Med. Assoc.*, 1924, 83, 1684.
[2] Halliburton: "Text-book of Physiology," 1921, 392.

Leonard Hill, also wrote soon after:[1] "It is impossible to give a satisfactory account of the gaseous exchange between the blood and the alveolar air."

Since then, however, considerable careful work and more perfect instruments, Krogh's micro-aerotonometer, for instance, have added factors which eventually may indicate that the process of diffusion is part of the respiratory process as a whole. Yet, various other factors confuse the whole question. Thus, the absorption of oxygen by the pulmonary blood from the air in the alveoli persists when the oxygen pressure is almost nil (Paul Bert); a strangulated animal exhausts all the oxygen out of the air in its lungs (Müller), asphyxiated animals show but traces of oxygen in their lungs (Setschenow and Holmgren, Zuntz and others.) To meet these and other recorded objections to the simple diffusion doctrine, Bohr, of Copenhagen, suggested that the pulmonary epithelium might itself secrete oxygen into the blood and excrete carbon dioxide into the alveolar air. Haldane and Lorraine Smith, Douglas and others have favored this theory. But it is evident that this oxygen must be derived from some exogenous source. Again, the swim bladder of fishes, the *Gadidae* to which the familiar cod-fish belongs, is given as example, because, if emptied and gradually refilled with air, the bladder will be found to contain nearly 80 per cent. of oxygen. But it has not been demonstrated that the epithelium of the bladder secretes oxygen, while there is ground for belief that a pharyngeal apparatus over which the air absorbed from the sea water passes on entering the bladder, undergoes a change, while the oxygen becomes concentrated to supply the animal with this gas, particularly in the depths, where the air is scarce.

On the whole, it is self-evident that despite much laborious and patient study, the initial step of the respiratory process which occurs in the lungs, still stands on a very uncertain foundation. That the terminal process in the tissue cells should itself be obscure becomes a self-evident consequence. Hence Halliburton's deploring remark concerning this function and

[1] Hill, Leonard: *Med. Press and Circular*, 1906, 166, 13.

its consequences as regards pathology, which includes the whole domain of applied medicine.

Might some important factor, overlooked by the host of investigators who had given their attention to the subject, not help to elucidate the problem of pulmonary and tissue respiration as a whole?

Twenty-five years ago[1] I pointed to the adrenals as the dominant organs in the whole process of respiration, pulmonary and cellular. Since then, much evidence, both direct and indirect, though sterile and useless in the orthodox field, found a ready nidus in my own interpretation of the respiratory process. Again, in a recent research by Dr. F. S. Hammett,[2] physiologist at the Wistar Institute, covering two years, the dominant rôle I had attributed to the adrenals was sustained in every particular. Other authorities have spoken similarly.

The space at my disposal does not permit me to submit more than the bare conclusions reached. The confirmatory evidence, in fact, will require a large volume, which is in course of preparation. Summarized, this evidence will show that the adrenals are the actual thermogenic organs which supply the respiratory pabula to the tissue cells and that the thyroid gland and its parathyroid glandules serve to accelerate thermogenesis, *i.e.*, the liberation of heat energy.

The Endocrine Thermogenic Mechanism

The main organs of this mechanism, which carries on tissue respiration, are illustrated in the annexed plate. Their individual, though correlated functions are as follows:

1. The Adrenals contribute three substances. Their *medulla* secretes an oxidizing, catalytic enzyme that I have termed *adrenoxidase*, forming part of the hemoglobin molecule, the active principle of which is the familiar epinephrin or

[1] Sajous, C. E. de M.: "Internal Secretions and Principles of Medicine," 1903, i; 1907, ii and *Phila. Med. Jour.*, 1903, Mar. 7.

[2] Hammett: Discussion at the meeting of the Association for the Study of Internal Secretions, Atlantic City, May 26, 1925.

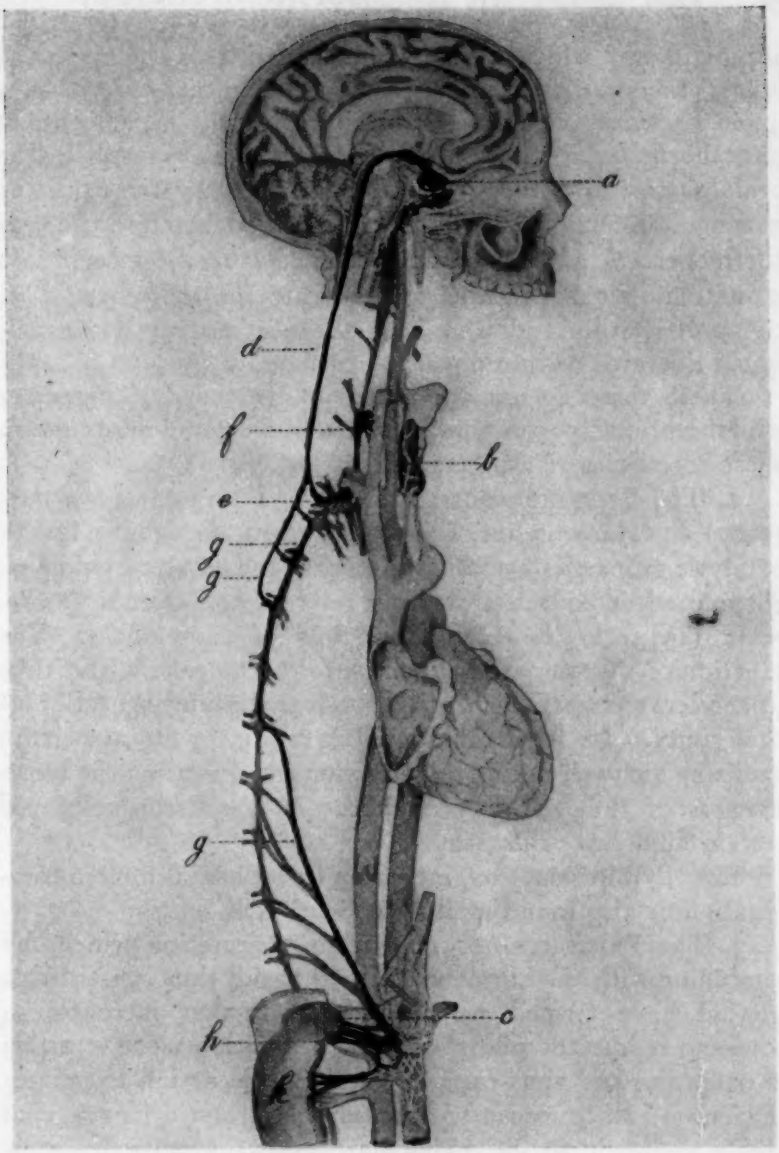

PLATE I.—The endocrine system as mechanism of tissue respiration or thermogenesis. (Sajous).

a, Pituitary and basal nuclei containing the thermogenic and renal centers, which govern the endocrine system. *b*, Thyroid and parathyroid glands, and *e* and *f*, their nerve-paths from the pituitero-nuclear centers. *c*, Adrenals and their nerve-path from the pituitaro-nuclear center via *d*, the cervical cord, and *g*, the greater splanchnic; *a*, *d*, *g*, *h* and *k*, nerve-paths from the pituitaro-nuclear center to the adrenals and kidneys respectively, via the spinal cord, bulb, and greater splanchnic.

adrenalin, and which takes up the oxygen from the air in the lungs; their *cortex* contributes two substances, *a*, *lecithin*, a lipoid or waxy phosphatid containing nearly 4 per cent. of phosphorus (which occurs in the heart as *cuorin* and in the brain as *cephalin*) and which when oxidized by the oxidizing enzyme adrenoxidase liberates heat and is therefore the true thermogen; *b*, *cholesterin* or better, *cholesterol*, since it is a monatomic alcohol, serves to moderate the thermogenic activity of lecithin and thus keep the heat energy which this lipoid liberates, within physiological bounds.

These three agents are taken up by the red corpuscles while the blood passes through the adrenals, and are found in these corpuscles as well as in all tissue cells.

2. The THYROID contributes what I termed *thyroiodase* (1907) in which iodine, its active agent, is endowed with catalytic properties by the admixture of the catalytic oxidizing enzyme adrenoxidase previously referred to. Kendall's *thyroxin* (1914) is the active principle of thyroiodase. The function of thyroiodase is to activate thermogenesis and thus to accelerate metabolism and oppose the inhibiting action of cholesterol. Its iodine, rendered catalytic by its associated oxidizing enzyme, fulfills this function by increasing the sensitiveness of the lecithin-phosphorus of the tissue cells and certain fluids, to oxidation.

The thyroid secretion, including its iodine in organic combination, is also found in all soft tissue cells.

3. The PARATHYROIDS contribute a secretion which, by combining with the hormone of the thyroid, thus constituting what I have termed *parathyroiodase*, further increases its power to render the phosphorus of the lecithin more sensitive to oxidation and thus raise its thermogenic action to greater efficiency. Its purpose is to insure hydrolytic cleavage of highly resistant toxics, particularly the spasmogenic intermediate wastes of metabolism, and also to provide for the formation of calcium phosphate in the bones and other tissues, Waldemar Koch,[1] of Chicago, having shown that calcium

[1] Koch, Waldemar: *Zeit. f. physiol. Chemie*, 1902, 36, 134.

becomes insoluble and is precipitated when lecithin is withdrawn from the blood—a result due to deficient heat energy.

4. The THYMUS is another gland, extending from the lower edge of the thyroid into the thorax, which, from my viewpoint, is concerned with thermogenesis. It is not shown in the plate, because it is only an accessory gland, which functions during development, to provide the excess of lecithin required by all tissues, the cerebrospinal system and skeleton particularly, during that period. The lecithin is supplied to all tissues by their special leucocytes, the nucleins of which owe their activity to their lecithin content.

5. The PITUITARY BODY governs the functions of the thermogenic mechanism, which includes the adrenals, thyroid, parathyroids and, in the developing child, the thymus.

In 1903 and 1907 [1] I submitted that the functions of the pituitary body included that of liberating nervous energy; that this gravitated in nerve paths up to the great basal nuclei in the tuber cinereum, and thence backward and downward to the bulb, spinal cord and sympathetic nerves to, among other structures, the various ductless glands. Time has sustained this conception. Briefly, the pituitary body is a powerhouse for the production of nervous energy used by the great basal nuclei or ganglia at the base of the brain.

ENDOCRINE THERMOGENESIS AS A DEFENSIVE PROCESS

The purpose of the heat energy liberated and controlled in the manner described, is to sustain the activity of the tissue enzymes, trypsin, lipase, etc., the efficiency of which, as is well known, depends upon the temperature to which they are subjected. It is these enzymes which carry on tissue metabolism. There is no such process as "tissue oxidation," as is now taught. Under normal conditions the heat energy liberated by the endocrine system suffices to sustain general metabolism, including the catabolism of wastes. When, however, pathogenic agents, an excess of toxic wastes, bacterial toxins,

[1] Sajous, C. E. de M.: "Internal Secretions and Principles of Medicine," 1903, i; 1907, ii.

etc., appear in the blood or tissue cells, thermogenesis, *i. e.*, heat production is increased in order to raise the activity of the enzymes, thus enabling them to break down the harmful substances defensively. The latter process manifests itself as "fever," a phenomenon which also has remained unexplained.

As previously stated, tissue respiration is the dominant factor of pathology. As herein interpreted, it eliminates confusing assumptions in all directions. Thus, in present day Medicine, it is believed that each of a multitude of agencies breeds disease specifically in its own way by acting directly on the tissues. With the defensive process considered as an exacerbation of the respiratory process the pathological changes in the tissues are not due to the foreign agent accidently present in them, but to the local defensive reaction this agent provokes. Each of these pathogenic agents presents special characteristics, chemical and structural; among these may be mentioned the wax covering of the tubercle bacillus and the various degrees of virulence which endow a pathogenic organism with more or less active resistance to defensive enzymes. All these cause the defensive process to resort to special means to protect the body, calcareous deposition, encystment, etc., if it cannot destroy outright the pathogenic agent. They are resources of the defensive mechanism itself, the bactericidal or antitoxic resistance being commensurate with the aggressiveness of the pathogenic factor.

Fever as a Defensive Exacerbation of Endocrine Thermogenesis.—Fever, another process the nature of which had never been explained, is the expression of the endocrine thermogenic mechanism when the latter becomes overactive in all tissues to defend the body against an infection, a toxemia, etc. Knowledge of this fact enables us further to realize that the endocrine organs are dominant factors in the defensive functions of the body, and moreover, that, as demonstrated by much experimental and clinical evidence, we can, by means of heliotherapy, organotherapy, etc., govern the

activities of the body's normal resistance to disease. As practical experience has also shown, this enhances materially our mastery of disease, if delay has not definitely compromised the issue through tissue destruction. When fever does not exceed 104.5° (40.3° C.) it is strictly defensive. Beyond this limit it may become harmful, as we shall see. If fever does not manifest itself adequately in a severe infection, the issue is correspondingly compromised.

We may, however, turn on at will the defensive current, when knowledge of the functions of the endocrines described guides our hand. Thus, when we were out a few days from Naples, some years ago, an epidemic of diphtheria broke out in the steerage which resisted the free use of antitoxin by the ship's able surgeon. The children buried at sea averaged three a night. Examination of those left showed them to be puny and debilitated. They were evidently unable to contribute, owing to deficient febrile reaction, their share of endocrine defensive products to their own blood, and the antitoxin alone was inadequate to save them. Personal studies having shown three years earlier that mercury was the most powerful pharmaceutic stimulant of the endocrine system we possessed, I advised giving each child, besides the antitoxin, 1/12 grain (0.0054 Gm.) of calomel every two hours, then every four hours. The deaths ceased at once, although the epidemic persisted.

Again, proper appreciation of the influence of the endocrines on tissue respiration may save life on the very threshold of death. Indeed, no one should be allowed to die of the effects of an infection to-day without being given the benefit of the efficient aid that its resources afford. I have termed "terminal hypoadrenia" a condition in which the adrenals, exhausted by the drain upon them of prolonged defensive activity, are unable to secrete enough of their oxidizing principle, adrenoxidase, to sustain life. Here, the cause of death is cessation of the defensive reaction. The injection of a few drops of epinephrin or adrenalin, the pharmacological active principle of the adrenal medulla, in a syringeful of saline solu-

tion, will cause a veritable resuscitation, often within ten minutes in such cases.

Advanced age favors materially a fatal ending in any infectious disease because of the gradual, though normal, diminution of functional activity of endocrine glands incident upon senescence. Plate II shown herewith illustrates the arterial supply of these glands at various ages. The three upper young-adult adrenals recall a bush in the spring while the two from aged individuals shown below typify the winter of life. We are dealing here with the foundation of longevity. The majority of elderly people who are said to die of pneumonia are in reality not victims of this disease but of a condition inappropriately termed "senile pneumonia," which is but failure of the adrenals, with edema of the lungs as true cause of death. Many valuable lives could be saved were the simple measures described above employed in these cases.

Excessive Fever or Thermogenesis (Hyperthermia) as a Cause of Blood and Tissue Destruction

The biochemical history of lecithin is very suggestive in hemolysis, *i.e.*, blood destruction. It is owing to the presence of this phospholipoid in the red corpuscles that these cells become vulnerable to the effects of venoms. The venom so sensitizes the lecithin phosphorus that a sudden liberation of heat energy occurs, which renders the blood cells vulnerable to the digestive action of the tissue tryptic-enzyme. Billions of red corpuscles are destroyed in a few moments wherever the venom is allowed to reach. The late Professor Linossier,[1] of Lyons, wrote, in 1920, after a comprehensive study of the lipoids, that the prevailing idea as to "the general antitoxic rôle of cholesterol is purely imaginary," but that the phospholipoids, which include lecithin, render the red corpuscles vulnerable to the hemolytic effects of venoms, while cholesterol protects these corpuscles against the same action. We clinicians can add evidence to these experimental truths, for in pernicious anemia, cancer and other diseases in which hemolysis is evident, one of our sheet anchors is cholesterol.

[1] Linossier: "Les Lipoides dans l'Infection et l'Immunité," Paris, 1920, p. 98.

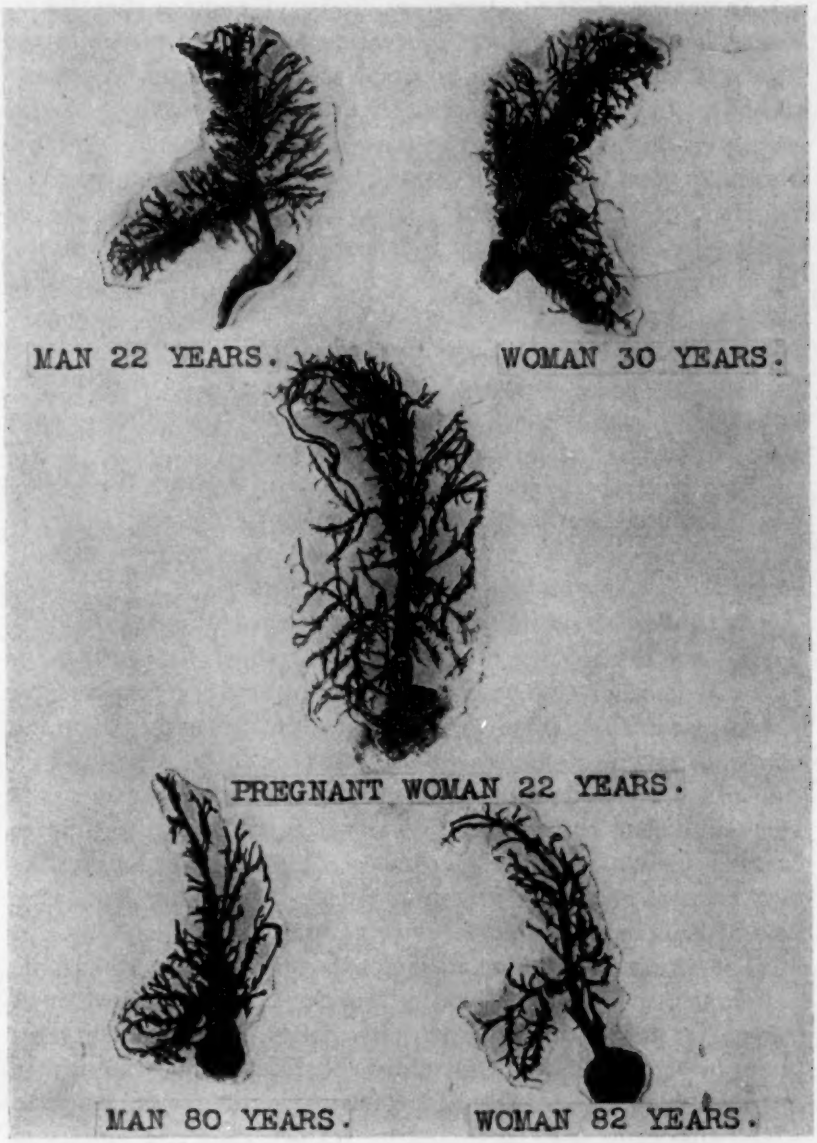

PLATE II.—Vessels of the Adrenals in the young and old. (Landau).
As age progresses, the adrenals gradually decline in functional activity, reducing correspondingly their thermogenic and defensive efficiency. (Sajous).

In hemolysis due to toxemias, however, such as that due to bacterial toxins, focal or general infections, typhoid fever, tuberculosis, cancer, etc., grouped under the term secondary anemias, the hemolysis is not direct, as in the case of venom autolysis, but indirect in the sense that it is due to excessive thermogenesis (particularly in the liver, when the temperature is often 106° F.) of central origin, the pituitaro-nuclear fever centers or thermogenic mechanism being violently stimulated by the pathogenic agent. Oversensitized by the thyroparathyroid hormone, the lecithin is oxidized at an inordinate rate by the adrenoxidase also produced in excess, and the liberation of heat energy is increased in proportion. The proteolytic activity of the tissue enzymes being correspondingly augmented, the red corpuscles are broken down.

Hemolysis may also be due to factors acting differently; but the above summary will suffice to illustrate the fact that hemolysis, usually the most destructive forms, may be caused by endocrine thermogenesis.

As to the destructive effects of excessive endocrine thermogenesis on the tissues themselves, *i.e.*, autolysis, I have repeatedly urged that the prevailing obscurity concerning tissue respiration, fever, and the rôle of the enzymes in the course of febrile processes was responsible for a large proportion of untimely deaths. The morbid process in autolysis is very similar to that which prevails in hemolysis, the tissues, cardiac valves and cardiovascular endothelium, the serous membranes, peritoneal, pleural, pericardial, meningeal, etc., being subjected to autolytic destruction.

The direct destructive action of bacteria is usually incriminated, as, for instance, in valvular lesions. But how account for their production by various drugs, uranium nitrate, potassium bichromate, and many others?[1] All these agents are known to be capable of promoting a defensive reaction of the endocrine organs, including activation of the functions of the pancreas, and the production of trypsin. It is obvious that any similar or identical local or direct mode of action of

[1] Christian and Walker: *Boston Med. and Surg. Jour.*, 1910, 162, 901.

hundreds of different agents on the autolyzed tissues is not to be thought of.

Conversely, we have ample evidence to the effect that one or two physiological agents, the defensive trypsin or pepsin-like enzymes, are capable, through a local defensive process, of creating lesions in all tissues, including those observed in arteriosclerosis. Indeed, a proteolytic enzyme, such as the tissue trypsin, is familiarly known as a solvent of the tissues of any organ, liver, lung, kidney, heart, vascular tissue, spleen, stomach, intestine, etc.—if adjusted to the required reaction, slightly acid or alkaline, and under appropriate heat conditions.

In the body, we have seen, these required conditions are fully met by the endocrine thermogenic system. It supplies the heat energy required to endow the enzymes which all cells contain with adequate potency. When exaggerated, however, in order to destroy the pathogenic agent, whatever it may be, it succeeds well enough perhaps, but in doing so, the enzymes (usually tryptic) also liquefy or digest the tissues containing or underlying the harmful agent. Cirrhosis of the liver, for instance, is not only caused by alcohol, but also by a large number of agents; despite this, the endocrine autolytic process is the same for all these pathogenic factors. Various bacteria and poisons, we have seen, may cause destructive valvular or myocardial lesions; but the same endocrine autolytic process is the creator of the lesions by liquefying the valvular tissues while destroying the pathogenic organisms. The cardiac arterial blood is especially rich in endocrine thermogenic agents; hence the predilection of the mitral valves, the aorta and coronary system to atheromatous changes followed by fibrosis or sclerosis. The same process prevails in the entire arterial system, with arteriosclerosis as familiar result. Pulmonary cavities owe their origin to similar phenomena, the wax-protected tubercle bacillus calling forth a very active defensive local reaction in the pulmonary tissues, which are concomitantly liquefied or broken down. Infected bones and joints succumb to a similar process, as in fact do all tissues, even the modes of local repair being virtually the same in all structures.

All this applies as well to the brain and nervous system, but endocrine thermogenesis so profoundly clarifies prevailing knowledge of psychoses and neuroses that it was deemed advisable to devote a special section to these subjects.

Excessive or Deficient Endocrine Thermogenesis as a Cause of Mental and Nervous Diseases

In 1903, in the first volume of my treatise on the Internal Secretions, I wrote a section of seventy-two pages on the histology of the neuron, in which I pointed to lecithin and adrenal oxidizing substance in all cerebral and nerve cells as the source of nerve energy. All the participants in the endocrine thermogenic process are, in fact, the active functional agents in all nervous tissues: lecithin and cholesterin (cholesterol) in their myelin, and the adrenal principle adrenoxidase in their axis cylinders. It may be recalled in this connection, that Harvey, for obvious reasons, was unable to describe the circulation of the nervous system, and that when I pointed out in 1903 that it was the blood plasma laden with the adrenal oxidizing substance which circulated in the axis-cylinders—the path followed by tetanotoxin—it meant the discovery of the circulation in all nerves. A schematic study of the nerve-cell as an organ is shown in the annexed illustration, Plate III.

The dominant deduction imposed by these facts, however, is that the nerve and brain cells do not differ in their biochemical attributes from any other cell in the body, all organs being thus planned similarly as regards thermogenesis. This applies also to the manner in which they respond to toxic processes and become the seat of lesions, sclerotic areas, etc.

A multitude of pathogenic agents, toxins, wastes, etc., are now thought, for instance, to cause about 75 per cent. of the cases of insanity in our asylums, the intoxication psychoses in particular. Knowledge of the endocrine thermogenic functions, however, shows that it is only through the excessive metabolic activity that all poisons provoke in the brain cells that the maniacal or excitement symptoms are produced and

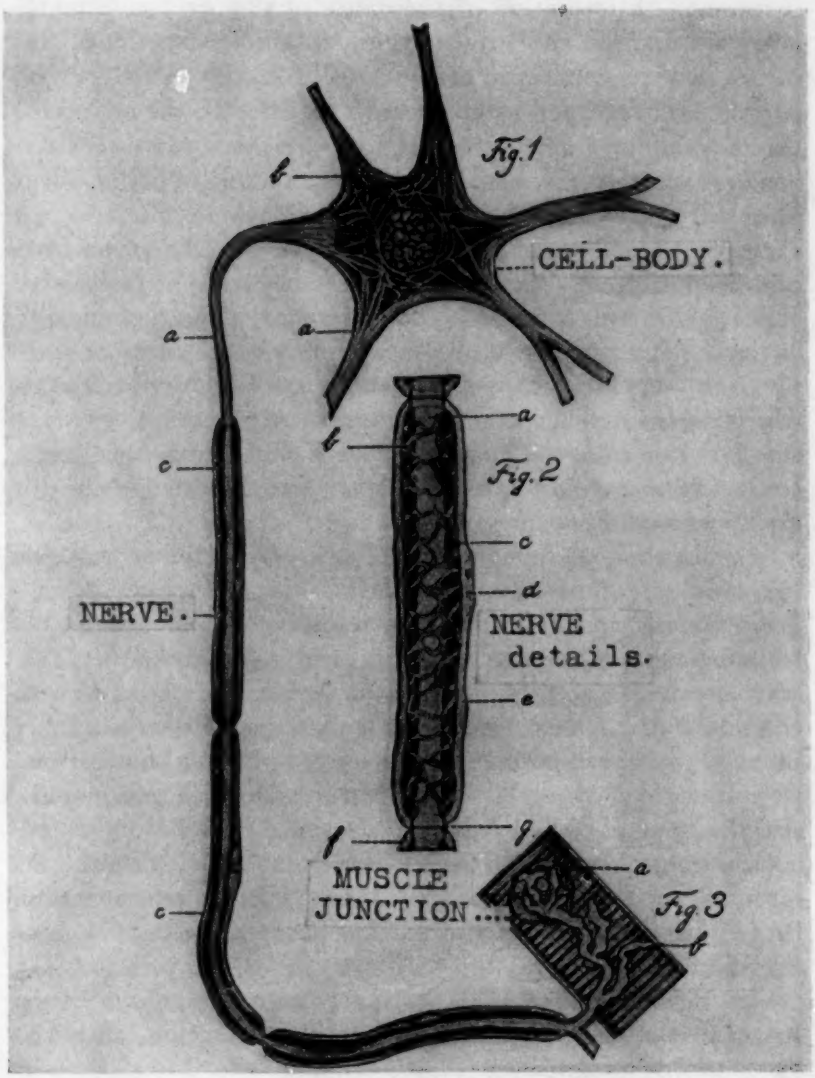

PLATE III.—The nerve-cell as an organ. (Sajous.)

The nerve-cell owes its functional activity to endocrine products similar to those present in other tissue cells *a a a*, axis cylinders and *c c c*, networks, all capillaries for adrenoxidase-laden plasma to the *c* myelin, containing lecithin and cholesterol.

sustained. Elimination of the cause of this excessive metabolic activity of endocrine origin, as ample experience has shown, will serve to cure many cases of insanity. In general paresis, still regarded as hopeless in text-books, the endocrine thermogenic and defensive reactions are inadequate to antagonize definitely the usual pathogenic factor, syphilis. But powerful vaccines, even disease inoculations, malaria for example, though used empirically, have already saved over one-third of these supposedly hopeless cases. The depressive psychoses or melancholias become the converse pathologically, of the manic or maniacal psychoses; however caused, we are invariably dealing with deficient activity of endocrine organs, which means deficient thermogenesis. A reduction of 60 per cent. of the cases of insanity treated sufficiently early is a conservative estimate of what adequate knowledge in this direction would procure.

In diseases of the spinal cord and peripheral nerves, the paramount feature of interest is that the endocrine thermogenic system, as activator of the defensive enzymes, accounts for the major lesions to which muscular paralyses are due. The so-called "fatty bodies" of the myelin sheaths of nerves, composed of cholesterin crystals and phosphatids commonly referred to in text-books of pathology, but of unknown function though theoretically dubbed "insulators," are naught else than our phospholipoid lecithin and our monatomic alcohol cholesterol, while the axis cylinders and their net-like extensions in the myelin carry the oxidizing adrenoxidase. Proteolytic enzymes are also present in the nervous system as elsewhere.

In infantile paralysis, or acute anterior poliomyelitis, for example, there is at first a local congestion, followed by local destruction of nerve tissue, the ganglion cells and their prolongations, their granular disintegration, leaving granules or globules which blacken osmic acid, the stain for the phospholipoid lecithin. After a time the local "inflammatory" process disappears and the paralyzed muscles supplied by the spinal segments involved show changes similar

to those which follow division of their nerve trunk. Many other forms of paralysis show similar disturbances of the central medullary sheaths, indicating that a common pathogenic factor, endocrine pathogenesis and proteolytic destruction or autolysis, have prevailed.

The Kidneys as the Excretory Organs of the Endocrine Thermogenic System

Plate I. opposite page 50, clearly shows, not only that the kidneys are in immediate contact with the adrenals, but also that they receive a copious nervous supply from the ganglia which supply the latter organs. All are derived from the greater splanchnic, thus suggesting that the renal excretion of wastes by the kidney is such as to keep pace with the catabolic production of wastes by the endocrine thermogenic system In 1903,[1] I submitted that agents which caused adrenal insufficiency and thereby inhibited metabolism—opium, copper, etc.—likewise inhibited renal activity, while those which stimulated the adrenals likewise caused a corresponding overactivity of the adrenals. An example of this fact is afforded by the action of mercury, which, as I previously stated, is one of our most active stimulants of the endocrine thermogenic system, and the extreme renal irritation observed in cases of poisoning by mercurials. As is also shown in plate II., the adrenals are greatly enlarged during pregnancy. Hence the care with which obstetricians watch the condition of the urine. Excessive thermogenesis, as indicated by the enlarged adrenals, though a physiological process in this connection, means excessive renal action.

More recently,[2] I reviewed this whole subject and was led to the conclusion that in the tuber cinereum, the nuclei which receive their supply of nervous energy from the pituitary body to the adrenals and other endocrine organs, there exists an additional path which, as I then wrote, "extends via the tuber

[1] Sajous, C. E. de M.: "Internal Secretions and Principles of Medicine," 1903, i, 59.

[2] Sajous, C. E. de M.: *Amer. Jour. Med. Sci.*, 1924, 167, 679.

cinereum, the bulb, cord and splanchnic nerves and renal plexus to the vessels of the kidneys." We have seen that fever is caused by irritation or lesions of the pituitaro-nuclear area and that the direct cause of this protective phenomenon is the endocrine thermogenic system. It is evident that this system and the kidneys are functionally related.

Conclusions

In view of the fact (1) that the functions of the endocrine organs have not been explained heretofore; (2) that my interpretation of these functions has alone been sustained by evidence covering all branches of Medicine during the last twenty-five years; and (3) that the rôle of all the biochemical bodies involved in these functions, though familiar components of the tissues, had also remained obscure otherwise than when interpreted by myself as factors of the said functions, the conclusion seems warranted that my labors have been the first to point out:

1. The fundamental functions of the endocrine organs;
2. The physiological process of tissue respiration;
3. The nature of fever and of its defensive function;
4. The nature of the morbid effects of excessive fever, hemolysis and autolysis and the pathological unrecognized importance of the latter;
5. The circulation of the nervous system; and
6. The identity of the kidneys as excretory organs of the endocrine system.

The examples outlined represent but a small proportion of the disorders which, as clinical experience has shown, the endocrine organs influence directly or indirectly. The functions of these organs I have submitted afford a solution for many problems in all departments of medicine which, so far, have only been met by pure and misleading conjectures. Moreover, by revealing the true nature of tissue respiration and its participation in our defensive functions, they tend to replace the prevailing empiricism by the utilization and in-

tensification of Nature's efforts in protecting the body against disease—in keeping with the most efficient resources of our day, those afforded by immunology, which have rid the world of many of its most destructive diseases.

Hence the fact that I regard the endocrine organs as a foundation for a scientific medicine in which demonstrable facts will replace many of the conjectures which now deprive it of its legitimate standing.

THE DEVELOPMENT OF BIOLOGICAL RESEARCH IN AMERICA

By C. E. McCLUNG

(*Read April 28, 1927*)

On an occasion like this, when one is asked to review the history of some human achievement, the temptation is great to present the contributions of a few outstanding men. Indeed, this is not altogether a wrong method, for whatever is accomplished by the group is typified in the performance of the individuals composing it. One thinks not so much of what the Greeks contributed to the development of natural history as he does of the outstanding individual work of Aristotle. The many who labored to unravel the enigma of organic relationships are often forgotten in the luster which surrounds the name of Darwin. If a careful enough selection be made, the history of individual efforts will mirror sufficiently well the performance of the times they represent.

The achievement of the unusual man is, however, in many ways a summation of manifold minor efforts on the part of less fortunate or able investigators. In whatever way we approach the subject, therefore, we come back ultimately to the work of individuals.

Throughout the history of any subject, moreover, there run tendencies which indicate the course of its development. Such tendencies often find their likeness in the history of the individual worker. In thinking over the subject of this address I recalled the autobiographical sketch of Agassiz, and on re-reading it I was once more impressed with the close parallel between the experiences of the individual and those of his group—an ontogenetic-phylogenetic parallel in the psychological realm, mention of which may happily escape a

controversial fate. The brilliant Swiss-American naturalist in reviewing his early career for the benefit of his students, among other observations sets down the following:

> I am conscious that at successive periods of my life I have employed very different means and followed very different systems of study. I may, therefore, be allowed to offer the result of my experience as a contribution toward the building up of a sound method for the promotion of the study of nature.
>
> At first, when a mere boy, twelve years of age, I did what most beginners do. I picked up whatever I could lay my hands on, and tried, by such books and authorities as I had at my command, to find the names of these objects. My highest ambition, at that time, was to be able to designate the plants and animals of my native country correctly by a Latin name, and to extend gradually a similar knowledge in its application to the productions of other countries. This seemed to me, in those days, the legitimate aim and proper work of a naturalist.
>
> . . . I did not then know how much more important it is to the naturalist to understand the structure of a few animals, than to command the whole field of scientific nomenclature.
>
> . . . I perceived that while nomenclature and classification, as then understood, formed an important part of the study, being, in fact, its technical language, the study of living beings in their natural element was of infinitely greater value.
>
> No thoughtful naturalist can silence the suggestions, continually arising in the course of his investigations, respecting the origin and deeper connection of all living beings; but he is the truest student of nature who, while seeking the solution of these great problems, admits that the only true scientific system must be one in which the thought, the intellectual structure, rises out of and is based upon facts . . . He is lost as an observer who believes that he can, with impunity, affirm that for which he can adduce no evidence.

The periods which Agassiz detected in his life experience were thus determined by the natural reaction of the developing mind to the conditions in which it found itself. The first impulse was to discover all within reach; the second, to apply names which had been used by others to designate the known forms and to invent new ones for those which were unfamiliar; third, to determine the nature of what was found; and, finally, from known facts, to form opinions regarding the origins and relations of living things. Although at first

placed in surroundings conducive to work of advanced character, the natural reaction of the young mind asserted itself. Later, when more mature judgment had developed and the conditions of life had changed, Agassiz found himself in the rôle of a founder of museums and laboratories. So far as the development of his subject went at the time, Agassiz also advanced, and, as a true student, pointed out lines for new movements.

When we look back at the history of biology in this country, we find a general parallel to the course of this individual worker. The early days were those of exploration and discovery, of adventure and romance, when men went forth into a strange and new country and found in the vast stretches of unexplored field and forest a rich and varied life, which constituted an irresistible attraction for their adventurous spirits. The most obvious and pressing need was for a knowledge of the extent and distribution of New World plants and animals, and this dictated, in large measure, the character of the biological work to be done. As with Agassiz in his youthful days, the urge to explore, to collect and classify, was insistent in the minds of American biologists. Here we find the names of men drawn from many countries of the Old World—Wilson, the Scotch weaver; Rafinesque, the cosmopolitan of Franco-German parentage; Bonaparte, the nephew of the great Napoleon; Audubon, of French parentage, and Lesquereaux, also of French descent. All of these and many others were drawn by the rich promise of the New World to a study of its many forms of plant and animal life. It is interesting to observe how many times these adventurers, overcome by their emotions of wonder and surprise, burst forth into poetical expression. It was indeed a time of youthful enthusiasms.

But not all of the early biologists were of the type of adventurers. Many of our most stable and profound minds showed a lively interest in scientific matters. The list of our early national presidents would be also a list of men interested personally in the development of scientific research. Wash-

ington was as much concerned with the creation of a national university as with almost any other enterprise of the country which he fought to establish—so much so that he left a part of his estate to found such an institution. Jefferson's connection with the University of Virginia is another evidence of this interest in opportunities for intellectual growth on the part of our founders. But Jefferson, in addition to this general concern for education, was himself profoundly interested in biological research. As President of the American Philosophical Society he found great relief from the problems of statecraft in association with men who worked in various departments of science. It is recorded that in the midst of a governmental crisis he left Washington for Philadelphia with an exhibit of fossil bones which he delighted in explaining to members of the Society. John Adams was equally concerned in the establishment at Boston of an institution which would serve the same intellectual purpose there that the American Philosophical Society was doing in Philadelphia, from which circumstance there arose the American Academy of Arts and Sciences. It is perhaps not a mere coincidence that all of these eminent statesmen and others saw the significance of scientific knowledge in the development of the new country. One can but regret that there has come about a separation between the interests of leading men in statesmanship and in science. It is perhaps inevitable in the growing complexity of modern civilization that this should be so, but it is none the less unfortunate.

A natural continuation of the efforts of individuals to determine what was new and valuable in the flora and fauna of the country appeared in the form of extensive surveys such as the Lewis and Clark expedition promoted by President Jefferson. In part these were furthered by army officers who were stationed at military posts throughout the country and who found their spare time interestingly spent in exploring the neighboring country, The Union Pacific railroad survey also resulted in a large publication with descriptions of many new forms. Gradually these efforts led to the formation of

organizations within state and national governments which have been continuously in operation down to the present time, and which have done much to extend the boundaries of the fields first explored by individual workers. While these have been more geological than biological in their tendencies, they have made valuable contributions in their paleontological discoveries especially.

The second period of Agassiz's experience in a general way again parallels that of biology in America. The accumulation of a large number of specimens and the need for their care and determination, led to the establishment of museums and botanical and zoölogical gardens. Here, as in many other cases of new biological movements, Philadelphia was the center. The Museum of the Philadelphia Academy of Natural Sciences, established in 1812, was the first of such organizations, and the Zoölogical Garden, started in 1859, was an initial effort. Going much further back was the Botanical Garden of the Bartrams which was the earliest attempt at organized study of our biological problems. It served also as an important link between early biological investigators in America and those of foreign countries. The international connections thus set up in the early days have, to a considerable extent, been carried forward by the Smithsonian Institution and the U. S. National Museum, with which it is connected.

This country is now rich in museums and gardens of the very finest character. It is only necessary to call to mind the American Museum of Natural History in New York, the New York Zoölogical and Botanical Garden, and the Arnold Arboretum, to realize the extent of our resources in this direction.

Agassiz's third phase of personal experience, that of trying to find the real nature of a living thing, is reflected multifariously in the activities of his followers. It is much easier to learn what animals and plants there are in the world than it is to discover how one of them is really constituted and how it operates. Such studies require apparatus and equipment

of varied and expensive character and laboratories in which to house them. To a very large extent these laboratories become a part of our institutions of higher learning. Their development was therefore conditioned by the character of these institutions.

The early history of education in this country was largely dominated by the classical ideals of the English college. Science was obliged to fight its way for a place in the curriculum, but once established, developments were rapid, and even in the high schools of the present day splendid biological laboratories may be found. At first college laboratories were of small importance, but as biology has become more complex they have taken on a more and more important position until at present their character largely determines the nature of our biological progress. While in the early days it was possible for a man of little learning, impelled by an interest in nature to go forth and make valuable contributions to biology, at the present time this is quite impossible. Biology has become an exceedingly complex subject and its ramifications extend far into the fields of physics and chemistry. It is necessary for the young biologist to get his training where all of these cognate subjects may be found. The university laboratory is therefore the place where biologists, going, not only into fields of applied science, but also into research careers, are developed. Realizing this, one cannot but wonder at the foresight and wisdom of men like Washington, Jefferson and Adams, who perceived the college and university to be a basic element of our modern civilization.

Not only is the university laboratory practically the only source of human material devoted to biological research, but for many years it has been the place where the largest proportion of original work has been accomplished. But with the increasing size of universities and the demands made upon the instructors for teaching large numbers of students, together with the rapidly augmenting complexity of the subject, the time available for research has become more limited. There is, however, no lessening of the pressure for new knowledge,

and as a result there came into existence a new type of biological institution, the research laboratory, where no instruction is given, but where the time of the workers is devoted entirely to original investigations. One of the earliest of this type is the Wistar Institute of Philadelphia. More recently, the Bussey Institution, and the laboratories established by the Carnegie Institution and the Rockefeller Institute, have taken their place in this class. For certain types of investigations, where a concentrated attack upon a problem or series of problems is required, uninterrupted by duties of teaching or administration, these research laboratories are of the greatest value. Biology in this country is fortunate in the possession of such opportunities as they present.

While speaking of institutions of this character we should not omit mention of certain government laboratories such as the ones found in the Department of Agriculture, the Bureau of Fisheries, and the Biological Survey, where, along with much applied work which the circumstances demanded, there have gone forward extensive and valuable investigations in pure science.

We may see in the foundation of laboratories and museums a further exemplification of the parallel in experience between the individual as represented by Agassiz and the group in which he found himself. When he came to this country his original interest lay in the classification of new materials and their study. Very soon, however, he was impelled to concern himself with the establishment of a museum where this material might be conserved and studied, and later, on the island of Penikese, with the inauguration of a new movement where investigators might be developed. In many respects, the personal contribution of Agassiz was the first step in a series of developments which led to the present fortunate situation in our American institutions. Although mention is not made by Agassiz in the autobiographical sketch I have quoted, of the influence exerted by other scientists upon his own career, a study of his life reveals to how large a degree personal relations affected him. Indeed, it may seriously be

questioned if his choice of a new home was not largely inspired by his reaction to the people he met in this country. In a letter to Milne-Edwards he wrote:

> Naturalist as I am, I cannot but put the people first,—the people who have opened this part of the American continent to European civilization. What a people! But to understand them you must live among them. Our education, the principles of our society, the motives of our actions, differ so greatly from what I see here, that I should try in vain to give you an idea of this great nation, passing from childhood to maturity with the faults of spoiled children, and yet with the nobility of character and the enthusiasm of youth. Their look is wholly turned toward the future; their social life is not yet irrevocably bound to exacting antecedents, and thus nothing holds them back, unless, perhaps, a consideration for the opinion in which they may be held in Europe.

We have here perhaps an evaluation of national character which indicates our most important resource. The importance of personal relationships can not be over-valued. It may be well, therefore, to inquire into this aspect of our subject.

It would surely be a poor picture of biology if the efforts made by the workers in this field to associate themselves together were not considered. Although much has been accomplished in a limited time to promote biological science by contributions of facts and theories, perhaps the greatest achievement of American biologists is the development of a common understanding and sympathy through organizations which have been created for this purpose.

One of the most outstanding examples of this is the Marine Biological Laboratory at Woods Hole, Mass. In a way a direct descendant of the original Penikese laboratory of Agassiz, this is in truth a natural development, reflecting the growth of a feeling of common interest and interdependence which has manifested itself through the years since 1888 when the laboratory first came into existence. Here, each summer, there are gathered together investigators from institutions scattered over the whole country, and in recent years, from many other countries. In this small village scores of workers

live and work together, and in this intimate association have learned to know the problems which have grown up in the different departments of a subject which daily becomes more complex and difficult. Because of this community of spirit we see nothing of the small jealousies and unworthy suspicions which often show themselves when workers lack personal acquaintance and intimate understanding. To this one place are brought the accumulated experiences of biologists from every sort of institution which the country supports, and these are made available to all who choose to take advantage of them. The man with a new idea, even though it be merely in the developmental stage, does not hesitate to place it with whatever facts he has in its support, freely before all those interested, with a confident feeling that not only will no unfair advantage be taken of his confidence, but that whatever there may be of supporting evidence will be brought to his attention. This is a resource of untold value which has many times demonstrated itself in concerted attacks on difficult problems and has finally resulted in so establishing the institution in the regard of those interested in biological development as to place it on a financial basis which will enable it to look confidently to the future. It is owned and managed by the biologists themselves and immediately reflects their judgment with regard to any new procedures which may be brought forward to provide new advances in biology. It represents also coöperation on the part of educational institutions which find in its presence a ready means for securing the best opportunities for biological research in the summer and at other seasons when this may be required in other surroundings. This laboratory is now the outstanding example of a marine biological laboratory, and is practically the only instance of one so organized.

While the Woods Hole Laboratory is the most conspicuous of our marine institutions, there are a number of others like the one at Cold Spring Harbor, and at Mt. Desert on the Atlantic Coast, and the Laboratories of the University of Washington, California and Leland Stanford, on the Pacific

Coast. Many other of our inland universities have similar laboratories established at fresh water lakes. It would be hard to overestimate the contributions which such institutions have made towards our biological development.

One of the good results coming from the recent World War was the organization in Washington of the National Research Council. This has very large accomplishments to its credit so far as biology is concerned. In many ways it has contributed to the community of understanding which the Woods Hole Laboratory has done so much to foster. By bringing men together for the consideration of definite problems, under conditions where the collective judgment could be best obtained, it has succeeded in promoting the organization of a number of most important institutions. Among these may be mentioned the Tropical Laboratory at Barro Colorado Island, the Tropical Plant Research Institute and the Crop Protection Institute. Besides these definite institutions it has contributed materially to securing financial support for the Woods Hole Laboratory and also towards the formation of the Union of American Biological Societies.

Unlike the chemists of this country, the biologists have been associated together in a large number of smaller organizations. When it became necessary to take collective action on problems of interest to all biologists this weakness manifested itself. As a partial remedy for this there was organized the Union of American Biological Societies which comprises among its membership twenty of these smaller organizations. The first achievement of this body was the establishment of an international system of biological abstracts. The rapid and extensive development of biology has resulted in the publication throughout the world of some 4,000 separate periodicals in which annually there appear in excess of 40,000 different papers. This mass of literature, for many, has become quite inaccessible, and the need for an introduction to it through an abstract system had become so evident that determined efforts had to be made to meet it. Again through the assistance of the National Research Council, financial support was found

for carrying forward this important enterprise and it is now definitely in operation. Like so many other biological enterprises it also has found its home in Philadelphia.

It is a difficult if not impossible task in so brief a time to make record of the history of biological research in America. In the outline just drawn there are sketched against the background of one man's scientific life certain movements in the general advance which leads from camp and barracks, through the executive mansion of our presidents and so on down through museum hall and primitive laboratory to our present splendid and varied equipment. It is a long advance and a creditable one. In certain departments there are achievements which rank with the best. Because of fortunate circumstances and the skill and devotion of a few unusual men like Leidy, Cope and March, American vertebrate palæontology early took a commanding position in the world's literature on the subject. From the time of Audubon and Wilson, studies in ornithology have been creditable. In more recent times American contributions in genetics, cytology and experimental development rank high. Much taxonomic work of great value has been accomplished. We have done well in the organization of agencies for promoting investigations and for securing records of them. We have been diligent and patient and sometimes even brilliant in learning the range and character of plant and animal groups. We have done much that is worthy in elucidating the nature and behavior of living things. In such efforts we have reflected the characteristics of a young and inquiring people who, as Agassiz said, have "the nobility of character and enthusiasm of youth." To carry on the investigative torch we are raising up a growing army of young students of nature. In all that has to do with the acquisition of new facts, the discovery and preservation of materials and their artistic display, the promotion of group understanding and action, and the planning of new methods and practices we have done all that might reasonably be expected.

But in that speculative field which Agassiz speaks of as

an endeavor to know "the origin and connection of all living beings" we do not rank so high. There has been among us no Darwin or Lamarck. Since, as it would seem, the individual partakes of the nature of his kind, it is perhaps not to be expected of a youthful people intent upon arriving as quickly as possible at the command of a great new country, that it should bring forth the product of a ripe and settled civilization. Contemplation does not keep company with acquisition. To play an honorable part in the search for underlying principles we must attain a spirit of repose and a more profound art of synthesis. Such attributes come in part from the seeking. We must desire them greatly before they are granted unto us.

It may be that the time is near at hand when we shall add more fully our proper contribution to biological speculation. The accumulation of new facts throughout a wide range of most diverse subjects now overwhelms us. There is urgently needed some generalization which shall mold our present knowledge into coherent form so that it may serve as a new point of departure into still more remote and difficult regions of the unknown. That we may round out a worthy bicentenary of biological research by such a contribution is, I am sure, the wish of all who labor in this present subdivision of the domain of knowledge.

ROGER BACON

By R. B. BURKE

(*Read April 28, 1927*)

I AM sometimes asked by newspaper men what bearing, if any, the work of Roger Bacon has upon modern life. I interpret this question as a formal challenge and forthwith proceed to defend my old friend Roger somewhat as follows: Are you aware, I ask, that no less a person than Columbus wrote a letter in 1498 to Ferdinand and Isabella in which he quotes a passage from the section on Geography in the "Opus Majus" of Roger Bacon? Columbus found this passage of Bacon in a book famous in its day entitled "Imago Mundi," which Humboldt remarks "exercised a greater influence on the discovery of America than did the correspondence with the learned Florentine Toscanelli." Are you aware, I ask further, that previous to the publication of Bacon's great work the belief was general that the Caspian sea was a gulf of the Arctic ocean? It is characteristic of Bacon's scientific method of approach that before drawing a conclusion he made a careful search for facts. Two members of his own order, the Franciscan missionaries Rubruquis and Carpini, had made reports of their travels, and from a careful study of these reports Bacon was able to state definitely that the Caspian is a great inland sea. How important a correction was thus made by Bacon in the map of Asia is obvious.

Bacon lived in an age when learned men were interested primarily in ideas. They discussed with intense earnestness, frequently with intense bitterness, the question of universals, individuation, substance, matter and form. Bacon also as a schoolman was profoundly interested in these burning questions of his day, and took no mean part in them. But Bacon with a clarity of vision centuries in advance of his age saw at

once that a dialectic of this kind was practically useless in searching out and discovering the secrets of nature. In one of the noblest efforts ever made on behalf of science Bacon in the sixth section of the "Opus Majus" makes an earnest plea for what he terms experimental science. In substance he says to the thinkers of his day, "You are intensely interested in the world of ideas, but you seem to forget that there is also a world of facts. Your dialectic which is wholly deductive in its method will never serve as a key to the secrets of nature. You must begin by observing and learning facts: mere dialectic, mere argumentation alone must ever prove barren of results in the physical sciences. Learn facts by actual experiment: sound deduction must be based on careful induction."

The sixth section of the "Opus Majus" has the proud distinction of being the first treatise on experimental science ever written. Bacon did what no man before him had done, he devised a scientific method of approach which has become the working principle of all modern scientific investigation. Men, you will say, had experimented before Bacon. This is true, but which of them had the breadth of vision, the depth of discernment, to realize the tremendous power of the instrument he was employing? It remained for Roger Bacon to raise mere experimentation to the dignity of a science and to point out clearly that along this pathway alone could Science advance into the unknown. Had Roger Bacon no other claim to fame than his clear enunciation of this great principle, I for one, at least, would be prepared to acclaim him as the father of modern science. Bacon, as Dr. Bridges points out, was original in his conception of all knowledge as a unit and in his teaching that all the sciences are interrelated. It was Bacon's delight to explain one science by examples from another.

So far in advance was Bacon of his own age in these and in many other respects, that Whewell in his "Philosophy of the Inductive Sciences" is moved to remark, "I regard the existence of such a work as the 'Opus Majus' at that period as a problem which has never been solved." Whewell again pays tribute in the following words: "The 'Opus Majus' is a work

equally wonderful with regard to its general scheme and to the special treatises with which the outlines of the plan are filled up."

Dr. Bridges in his introduction to the "Opus Majus" says: "The 'Opus Majus' remains the one work in which the central thought of Bacon is dominant from first to last: the unity of science and its subordination to the highest ethical purpose conceivable by man."

After several years of study of the "Opus Majus" I ventured in a recent article to express my own opinion of Bacon's great work in the following words: "In its unity of purpose, in its enclycopedic range of subjects, in its clarity of statement, in its orderly arrangement of material, in its prophetic scientific vision, in its profound moral earnestness, the 'Opus Majus' must ever remain one of the few truly great works of human genius." I believe that it is within the limits of truth to say that no other great scientific work has ever been inspired by so lofty a purpose as that which ennobled the "Opus Majus" of Roger Bacon. Paradoxical as it may sound, science is almost as much indebted to Bacon for his failures as for his successful solutions of physical problems. In his "Optics," for example, we are indebted to him for important problems proposed and elucidated by him almost to the point of final solution. Bacon by his clarity of statement and wealth of practical observations greatly reduced the difficulty and materially aided the solution of such problems, some of which were not finally solved until recent years.

The phenomenon of the rainbow excited the scientific curiosity of natural philosophers from Aristotle to Bacon. The latter attacked the problem with his characteristic thoroughness by applying his experimental method. If you would know Bacon at his best as a thorough, patient, and brilliant investigator of natural phenomena, read his remarkable discussion on the rainbow. The problem, as you know, remained unsolved for three hundred years after Bacon and yielded only to the master mind of a Descartes. Descartes no doubt was greatly aided by Bacon's thorough discussion of

the phenomenon in all its phases and above all by Bacon's scientific method of approach.

Bacon was deeply interested in magnetism, an interest which he derived doubtless in no small measure from his old teacher in Paris, Peter Peregrinus, one of the earliest pioneers in the investigation of this subject. In accordance with his own scientific method of approach Bacon tried to correlate with magnetic attraction other phenomena which depend upon the mutual attractions exerted by bodies at a distance. In the section on Experimental Science, for example, he writes "if a sapling of one year's growth is taken, which springs forth besides the roots of the hazel, and is divided longitudinally, and the divided parts separated by the space of a palm or four fingers, and one person holds on one side the extremities of the two parts, and another similarly on the other side, always with an equal and gentle grasp, so that the parts are kept opposite each other in the same position they had before the division, within the space of half a mile [1] the parts of the twig begin to approach each other gradually, but with greater force at the end of the experiment, so that at length they meet and are united. The ends, however, remain apart, because they are prevented from meeting owing to the force exerted by those holding the parts. This is a very wonderful thing. For this reason magicians perform this experiment, repeating different incantations, and they believe that the phenomenon is caused by virtue of the incantations. I have disregarded the incantations and have discovered the wonderful action of nature, which is similar to that of the magnet on iron. For just as the one attracts the other because of the similar nature of the iron and the magnet, so do the parts in this case. Hence the natural force, which is similar in both parts of the twig, causes them to unite. If they were arranged in the required way, they would meet at the extremities just as in the middle and more quickly, as for example if the ends were minutely pierced and threads passed through the ends, so that they could be suspended in the air without hindrance. This

[1] Time taken to walk half a mile.

is true not only of hazel saplings but of many others, as in the case of willows and perhaps in that of all saplings if they were arranged in the required manner."

Professor Rodney H. True of the University of Pennsylvania very kindly performed this experiment at my request under practically the same conditions as those stated by Bacon, and found that the parts of the sapling actually approached and finally came together as Bacon describes. Professor True, while he rejects Bacon's deduction, expresses himself as greatly impressed, as I believe we all should be, by Bacon's breadth and power of mind in associating phenomena so widely separated as the mutual attraction of iron and the magnet and the apparent attraction between the parts of the sapling.

In this age of historical criticism and historical doubt the question is sometimes asked whether gunpowder was really discovered by Bacon. We are told on competent authority that gunpowder was known to the Chinese long before the time of Bacon and possibly to the Arabs also. There is no indication, however, that gunpowder was known to the nations of Europe before the publication of Bacon's work on the "Secret Processes of Art and Nature" in 1249. In this work Bacon gives the first formula ever published for the manufacture of gunpowder. The formula, it is true, is veiled under an anagram, but the anagram is easily worked out and gives us the following ingredients with the percentage of each: saltpetre 41.2, charcoal 29.4, and sulphur 29.4. The "Opus Majus," which was written some eighteen years later, throws but little light on this question. The late Professor William R. Newbold, however, was convinced that Bacon was thinking of the use that might be made of gunpowder in war when he wrote the following passage at the beginning of the "Opus Majus": "For by the light of knowledge the Church of God is governed, the commonwealth of the faithful is regulated, the conversion of unbelievers is secured and those who persist in their malice can be held in check by the excellence of knowledge, so that they may be driven off from the borders of

the Church in a better way than by the shedding of Christian blood."

In the section on Experimental Science there is a direct reference to explosive compounds in the following passage:

"Certain inventions disturb the hearing to such a degree that, if they are set off suddenly at night with sufficient skill, neither city nor army can endure them. No clap of thunder could compare with such noises. Certain of these strike such terror to the sight, that coruscations of the clouds disturb the sight incomparably less. Gideon is thought to have employed inventions similar to these in the camp of the Midianites. We have an example of this in that toy of children, an instrument as large as the human thumb. From the force of the salt called saltpetre so horrible a noise is produced at the bursting of so small a thing, namely a small piece of parchment, that we perceive that it exceeds the roar of sharp thunder, and the flash exceeds the greatest brilliancy of the lightning accompanying the thunder."

In this passage Bacon evidently has no idea of the subsequent use made of gunpowder in war, and merely suggests that it might be employed to terrify an enemy by the exceeding brightness of its flash and the horrible roar of its explosion. It is significant to note that in an age which we are wont to consider slow and backward, children scarcely twenty years after the publication of Bacon's formula for the manufacture of gunpowder were shooting off fire-crackers in different parts of the world.

In view of what precedes is it unreasonable to suppose that Bacon actually did make an independent discovery of gunpowder and gave it to the world in the anagram published in 1249? This assumption will explain satisfactorily the belief current in his day and for centuries after, that Bacon was the discoverer of this compound.

Bacon, as we have seen, attached great value to experiment in the discovery of the laws of nature, but he was no mere experimenter; he was a profound thinker and a theorist of the first rank. Read carefully, for example, Bacon's lucid expla-

nation of how force is transmitted through an elastic medium, and I venture to say that you will be astonished to see how closely his theory resembles our modern ideas on this subject. Bacon in his search for truth refused to be shackled by authority. He was absolutely fearless in propounding what he believed to be the truth. No name was so august in Church or State, no authority so revered, that Bacon did not venture boldly to combat error wherever he found it. Aristotle taught that the transmission of light was an instantaneous process, arguing that if it were not instantaneous we could perceive the lapse of time when light passed through the vast distance separating east from west in the heavens. Bacon did not hesitate to say that even Aristotle was not infallible and that in this particular instance he was clearly wrong. The fact that our human sense can not perceive so small an interval of time is not proof that such an interval is not possible.

The renewed study of Aristotle had done much to quicken and stimulate the intellectual life of the twelfth and thirteenth centuries, but the time was approaching when Aristotle was to prove a paralyzing, a deadening blight on all freedom and originality of thought. Bacon was quick to sense the danger and to sound a timely warning. To state that this warning was unheeded is only another way of saying that Bacon was centuries in advance of his times.

Bacon's scientific interest was a broad one and led him into many fields of inquiry. He made a thorough and illuminating study and comparison of the Jewish, Arabic and Christian calendars. He pointed out clearly the errors in the Julian calendar, and warned the Pope in all seriousness that unless a thorough correction and revision of the calendar were made, Christians would be eating meat at a time when they should be observing Lent.

Bacon was an ardent student of mathematics which he considered the basis of all scientific knowledge and the key to all the sciences. His study and investigation included both theoretical and applied mathematics. In the latter field Bacon was quick to distinguish matters of prime importance

from those of secondary interest. For example he points out the necessity of an accurate determination of the length of a line on the earth's surface corresponding to one degree. He urges the necessity of more accurate instruments in all fields of research and looks forward hopefully to great advances in knowledge from their use. In the "Opus Tertium" he says: "I sought the friendship of all wise men among the Latins: and I caused young men to be trained in languages, in geometrical figures, in numbers, in the construction of tables, in the use of instruments, and in many other necessary things. During this time I spent more than two thousand pounds in those things and in the purchase of books and instruments." This sum was a large one for those days, as Dr. Bridges points out, and must have been expended by Bacon prior to his entrance into the Franciscan Order, which imposed poverty on its members.

Bacon's prestige as a scientist has suffered greatly in modern times owing to his openly avowed belief in astrology. In judging Bacon, however, we must bear in mind that nearly all the great thinkers of his age were firm believers in this pseudo-science. Saint Thomas Aquinas in explaining the influence of the stars on human life uses nearly identical language with that of Bacon. The poet Dante and a host of other writers during the thirteenth century and the three that succeeded it, expressed an unshaken belief in astrology. The keen analytic mind of Bacon made an earnest effort to place astrology on a scientific basis by explaining its supposed phenomena by the great truth so clearly grasped by Bacon, that every body in the universe influences all other bodies. Bacon warns us that astrology has its limits beyond which as a science it must not be pressed. He says emphatically that astrology can predict the future only in the most general manner, not in specific instances. The general judgment, as he calls it, not the particular one, lies within the province of this science. Bacon illustrates his meaning by the following story which he tells in all seriousness, but which cannot fail to strike the modern reader as more humorous than convincing.

A certain subject of the emperor had offended his royal master, and fearing arrest went into hiding. He knew, however, that he could not long escape detection, because his master was surrounded by clever astrologers, who would quickly by their art find out his hiding place. Deciding to fight fire with fire he summoned to his aid an able astrologer, who advised him to fill a large caldron with water, place a stool in the middle of it and sit all day long on the stool. Sure enough the emperor called his astrologers to his assistance, who after consulting their art reported that the fugitive was at sea. The man, thanks to the false information furnished by the astrologers, escaped. This story Bacon uses as an example of the way in which the noble science of astrology is brought into disrepute by attempting to draw from it particular instead of general judgments. In the words of our friends, the physicists, such astrologers stretched the science beyond its elastic limits.

Bacon sounds a solemn warning that no real advance in knowledge can be made except along the pathway of intellectual freedom. Free yourselves, he cries, first of all from the shackles of every authority that is not worthy of your allegiance; refuse to be the intellectual slaves of habit or of prejudice; abhor the false conceit of knowledge. Authority, habit, prejudice, false conceit of knowledge—these four are the sources of all human error. Clear these away first and then proceed.

Such teaching is indeed most remarkable in an age when authority was paramount in every field of intellectual endeavor, and the false conceit of knowledge was rife in all the schools. Some critics have found in Francis Bacon an explanation for the mystery of a Shakespeare in the sixteenth century, but no one has yet found another Bacon to explain the mystery of a Roger Bacon in the thirteenth century.

For you who are engaged in searching out the truths of nature Bacon has a noble, an inspiring message. Yours is a high calling, upon you in large measure depend the enlightenment and elevation of the world; all truth as well as all

sciences are one; great spiritual truths are apprehended by a clear understanding and knowledge of the laws of nature; the God of nature will reveal in full measure his secrets to those alone whose life is stainless and whose heart is pure.

THE ACOUSTIC IMPEDANCE OF STRAIGHT CYLINDRICAL AIR COLUMNS IN BRASS TUBES

By A. E. KENNELLY and R. P. SISKIND

(*Read April 28, 1927*)

The object of this research has been the measurement of the acoustic impedance of air columns in brass tubes, by means of their reaction upon the motional impedance of a simple electrodynamic loud-speaker element.

Apparatus and Method

Figure 1 gives an axial or longitudinal section of the loud-speaker element used, known as the "Radio Magnavox," type *R* 3, model *B*. The aluminum diaphragm, *DD*, is clamped between two circular metal rings, C_1 and C_2, compressed by eight machine screws, *S*, which pass through them and the edge of the diaphragm. Between the circular clamping rings and the diaphragm are two annular paper washers, *W*, (thickness 0.031 mm., exaggerated in Fig. 1). The diaphragm (0.018 mm. thick. exaggerated in Fig. 1) is not quite flat but has two annular corrugations.

The suspended coil, *L*, is attached by an aluminum tripod to the under center of the diaphragm through a small brass screw and nut *m*. The coil has a plural-layer winding of fine copper wire. It hangs in an annular magnetic field between the poles *PPP*. The excitation of this field comes from the coil *AA*. The annular air gap is 1.6 mm. wide and 5.0 mm. high.

In order to facilitate the application of this element to the measurements here described, certain small mechanical modifications were made in the construction, as follows:

1. The original upper clamping ring was replaced by a new ring, C_2, so as to open the entire upper surface of the diaphragm to the acoustic tube.

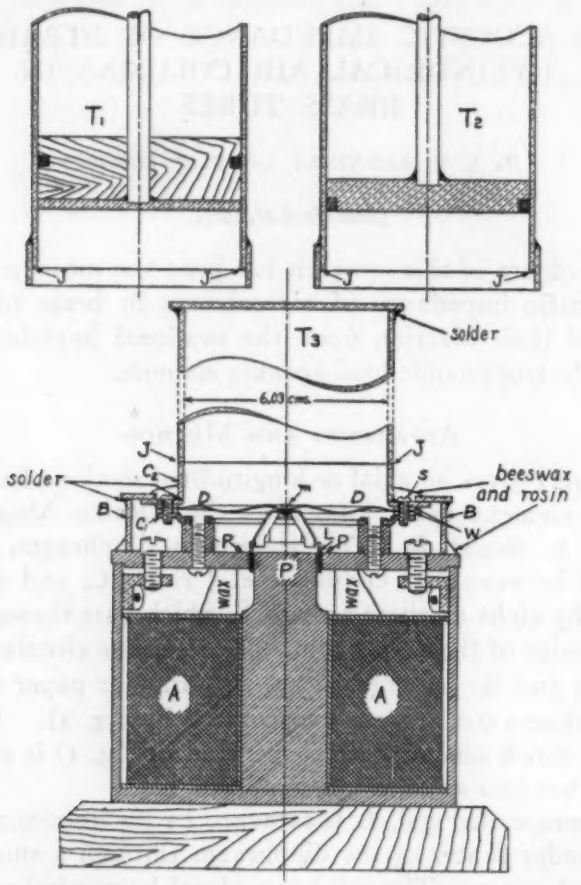

FIG. 1.—Axial section of loud-speaker element and air tubes used in tests.

2. The top of the small machine screw, *m*, was filed flat so as to present a suitable surface for the amplitude measurer, later described.

3. Paraffine wax was poured into the space indicated inside the shell below the diaphragm, in order to diminish the free space there.

4. A cap, *BB*, was fitted, as shown, to close the air space below the diaphragm. The seal was completed with beeswax and rosin.

5. The small transformer which is ordinarily used with this loud-speaker was eliminated and the terminals were connected directly to the leads running to the suspended coil.

Acoustic Loads.—The acoustic air columns tested were provided by brass tubes of different lengths. These were of standard seamless brass tubing, 2½ inches outside diameter (63.5 mm.) and No. 16 gauge (approximately 1/16 inch thick or 1.59 mm.) smooth, but not polished on the inside.

The first brass tube used had a brass-faced wooden plunger, which could be supported at any desired point in the tube. The length of the air column so secured could be measured to the nearest tenth of a millimeter, disregarding the uncertainty as to the base level of the corrugated diaphragm. T_1, Fig. 1, shows the details of this plunger and tube. It will be observed that the packing of the wooden plunger was about 1.3 cms. behind the brass face plate. As the seal was imperfect at this face, a very thin annular crevice was formed between plunger and tube. The minute air-space inside this crevice, at no place exceeding 0.5 mm. in width, was found to affect the electrical measurements markedly.

In order to reduce the size of this crevice of imperfect fit between plunger and tube, the construction was changed to that of T_2 in Fig. 1. Here the plunger was entirely of brass and the packing was brought as close as possible to the face. The fit of the plunger was also made as tight as was consistent with freedom of motion. Vaseline in small quantities was used, as a lubricant, with both of these plungers. In spite of all these precautions, however, the effect of the crevice between the tube and the plunger was easily detectible in the electrical measurements.

To eliminate this crevice completely, measurements were made with sealed-end tubes, using a brass disc soldered on to the end of a tube in place of the plunger. The desired lengths of air column were then obtained by cutting the tubes to the

required lengths. This arrangement is indicated at T_3 in Fig. 1. As will subsequently be detailed, the measurements then showed a distinct diminution in acoustic energy loss.

The seal at the junction, *JJ*, between the foot of the acoustic tubes and the tube part of the upper clamping ring, C_2 in Fig. 1, was made by very carefully machining the contacting brass surfaces and using a small amount of vaseline to render it absolutely airtight.

Test Circuit.—The alternating-current bridge used appears in Fig. 2. Fig. 2*A* is a schematic diagram; while Fig. 2*B* is the actual connection diagram. It will be observed that a Wagner ground connection was used.

In the bridge wire, *aa*, a two-stage amplifier, *Q*, was inserted for the magnification of the bridge wire current in the head telephones. It employed two Western Electric audio-frequency amplifying transformers, with sheet-iron cores, designed for maximum amplification at 1000~. This amplifier considerably improved the sensitivity of the bridge. It enabled the bridge to be set to detectible steps of 0.01 ohm, using a ten-to-one bridge ratio.

A five-watt triode oscillator was employed as the source of alternating current of adjustable frequency, which gave a frequency adjustment by steps of less than one cycle per second over the range used in this work (600~ to 700~). It was calibrated for frequency by the series resonance method, using a standard 0.1 henry inductance and a Leeds-Northrup, type 1070, decade condenser, which has a capacitance adjustable in steps of 0.001 μf up to 1.0 μf. Intermediate steps of 0.0001 were estimated. Resonant frequencies were computed to four significant figures. The frequencies obtained from the oscillator were found to be very nearly constant, over the small range of load employed.

Procedure

The method employed in all the tests was to measure the "motional impedance," $Z_m = R_m + jX_m$, of the loud-speaker vibratory system shown in Fig. 1, under various acoustic

loads and impressed frequencies. This included the measurement of the "free impedance," $Z_f = R_f + jX_f$, of the instrument in the presence of its load, and of the "damped

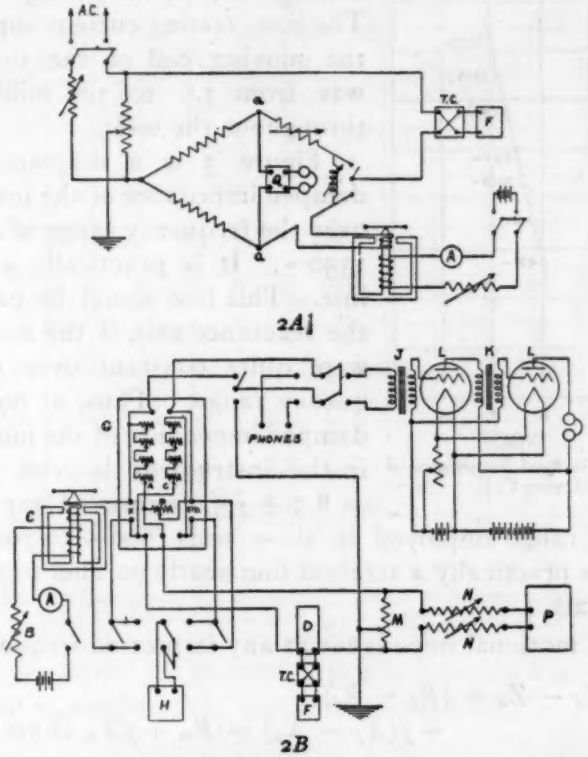

FIG. 2.—Connections and Test-circuit Diagram: *A*, 0–1.0 Ammeter; *B*, Beck Bros. 24-ohm slide wire; *C*, "Radio Magnavox" type R-3, Model B; *D*, General Radio Co. type 102-J decade resistance box as thermocouple shunt; *T.C.*, Vacuum tube thermocouple, normal current 10^{ma}, resistance of heater wire 44 ohms; *F*, Microvoltmeter; *G*, General Radio Co. type 193 Decade Bridge; *H*, General Radio Co. type 107 H variometer; *J*, Western Electric Co. type 201 G amplifying transformer; *K*, Western Electric Co. type 201 A amplifying transformer; *L*, UV 201 A Radiotrons; *M*, Beck Bros. 3320 ohm slide wire; *N*, Beck Bros. 990 ohm slide wires; *P*, Special vacuum tube oscillator; *Q*, 2-stage audio-frequency amplifier.

impedance," $Z_d = R_d + jX_d$, of the same.[1] This damped impedance, Z_d, was ordinarily obtained by repeating the free-impedance measurement, Z_f, with the d.–c. excitation removed from the electromagnet. The normal excitation

[1] Bibliography 12, Chapter VII., and also Bibliography 20.

current of the instrument, in loud-speaking service, is 1.0 ampere; but for these measurements, it was found more convenient to use a single smaller value of excitation current throughout, namely 0.5 ampere. The a.–c. testing current supplied to the moving coil of the instrument was from 3.6 to 3.8 milliamperes throughout the tests.

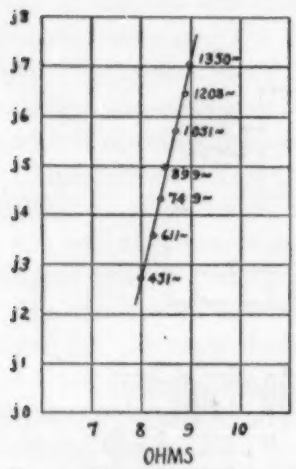

FIG. 3.—Damped Impedance of Moving Coil.

Figure 3 is a diagram of the damped impedance of the instrument over the frequency range of 451 ~ to 1350 ~. It is practically a straight line. This line would be parallel to the reactance axis, if the a.–c. losses were quite constant over this frequency range. Thus, at 899 ~, the damped impedance of the moving coil in the instrument is seen to be $Z_d = 8.5 + j\ 5.0$ ohms. Over the frequency range employed in these tests (600 ~ to 700 ~) this graph is practically a straight line nearly parallel to the reactance axis.

The motional impedance at any impressed frequency is

$$Z_m = Z_f - Z_d = (R_f - R_d) + j(X_f - X_d) = R_m + jX_m \text{ ohms } \angle. \quad (1)$$

At each observed point, the "damped resistance," R_d, was subtracted from the "free resistance," R_f, and the "damped reactance," X_d, from the "free reactance," X_f. These differences gave the two components, R_m and jX_m, of the motional impedance.

Figure 4 is the motional impedance diagram, obtained with both sides of the diaphragm open to the air; *i.e.*, the lower cap, *B* in Fig. 1, and the upper tube, *T*, removed. The resonant frequency of the vibratory element; namely, the circularly clamped diaphragm carrying the attached coil, was found to be 633.4 ~ under these conditions. It will be seen that the

motional impedance diagram closely approaches a circle passing through the origin and having its center on the resistance axis.

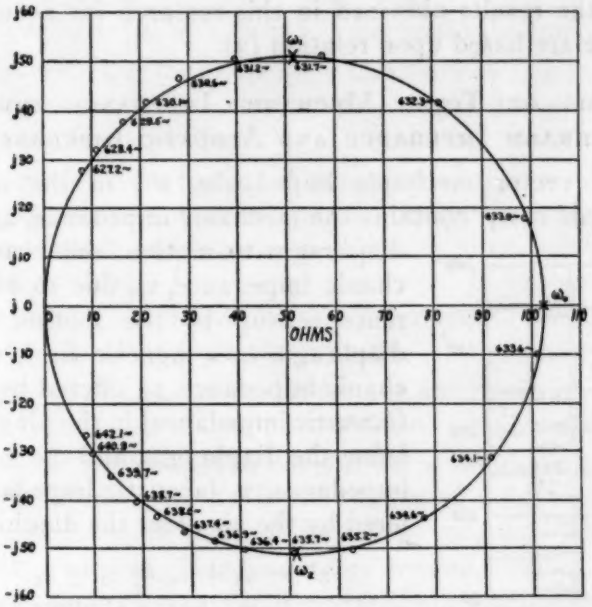

FIG. 4.—Motional impedance, Z_m, of vibratory system with lower cap, B, and upper tube, T, removed $I_{DC} = 0.5^{ma}$, $I_{AC} = 3.6^{ma}$, $T = 24.8°$ C.

The reciprocal diagram of Fig. 4 is given in Fig. 5, which therefore represents the "motional admittance,"

$$Y_m = \frac{I}{Z_m} = G_m + jB_m \qquad \text{mhos } \angle, \tag{2}$$

where G_m is defined as the "motional conductance," and B_m as the "motional susceptance" of the vibratory system. It consists of a straight line parallel to the susceptance axis at a distance of 0.0097 mho.

Conversion of Electric Motional Admittance, Y_m, into Mechanic Impedance, z''.—It is known that if A is the vector force factor of a vibratory telephone system, Y_m' the motional admittance of the system at any frequency, in abmhos, and z'' the mechanic impedance of the system, the last is defined

by [1]

$$z'' = A^2 Y_m' \qquad \text{mechanic abohms } \angle. \tag{3}$$

All of the results obtained in this research for acoustic impedance are based upon relation (3).

Analysis of Total Mechanic Impedance into Diaphragm Impedance and Acoustic Impedance

The vector mechanic impedance, z'', in the ordinary telephonic case,[2] contains the mechanic impedance, z_d, of the diaphragm to motion, the virtual mechanic impedance, z_v, due to eddy currents set up by the motion of the diaphragm in a mgnetic field, the mechanic impedance, z_1, offered by the air (acoustic impedance) in the air chamber *below* the diaphragm, and the mechanic impedance, z_2 (acoustic impedance) offered by the air *above* the diaphragm.

$$z'' = z_d + z_v + z_1 + z_2 \qquad \text{mechanic abohms } \angle. \tag{4}$$

Due to the construction of the loudspeaker element used in these tests, the diaphragm is outside the magnetic field, and z_v may be regarded as zero.

The mechanic impedance, z_d, of the diaphragm is known to obey the relation [3]

$$z_d = r_d + j\left(m_d\omega - \frac{S_d}{\omega}\right) \tag{5}$$

$$= r_d + jx_d \qquad \text{mechanic abohms } \angle.$$

Here the mechanic resistance, r_d, is

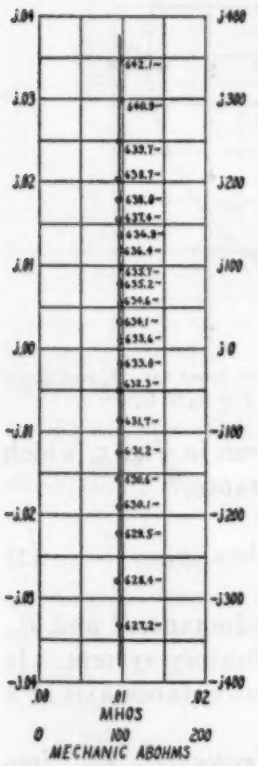

Fig. 5.—Motional admittance, Y_m, and mechanic impedance, z_d, of diaphragm with both lower cap, *B*, and upper tube, *T*, removed.

[1] Bibliography 12, p. 98.

[2] Bibliography 12, p. 171, eq. 182.

[3] Bibliography 12, p. 171, eq. 184.

practically constant over the range of frequency employed; but the elastic reactance, x_d, will vary. We have therefore

$$\begin{aligned} z'' &= z_d + z_1 + z_2 \\ &= r_d + jx_d + z_1 + z_2 \qquad \text{mechanic abohms } \angle. \end{aligned} \tag{6}$$

Measurement of the Diaphragm Constants.—The four characteristic constants of a circular diaphragm clamped around the edge are known to be [1]

(1) The vector force factor, A dynes per abampere $\angle$,
(2) The equivalent mass, m_d grams,
(3) The mechanic resistance or retarding force, r_d dynes per cm. per sec.,
(4) The elastic coefficient, S_d dynes per cm. of displacement

A is obtained from the formula.

$$A = \left|\frac{I_m Z_o'}{\omega_o \chi_{mo}}\right| \sphericalangle\ \beta^\circ \qquad \begin{matrix}\text{dynes per abampere } \angle, \text{ or} \\ \text{abvolts per cm. per sec.,}\end{matrix} \tag{7}$$

where I_m is the maximum cyclic value of the alternating current in abamperes, Z_o' is the diametral value of the motional impedance in abohms, ω_o is the angular velocity at apparent resonance in radians per second, and χ_{mo} is the maximum cyclic amplitude of vibration in centimeters of the diaphragm, at its center, at resonance, when Z_o' is observed.

With the ordinary telephone receiver, A is a plane vector quantity, or complex number, having the slope $-\beta^\circ$. In the case of this loud-speaker element, however, $\beta = 0^\circ$, and A is a real quantity. This greatly simplifies the conversion of motional admittance into mechanic impedance, according to (3).

The mechanic resistance is

$$r_d = \frac{A^2}{Z_o'} \qquad \begin{matrix}\text{dynes per cm. per sec.,} \\ \text{or mechanic abohms.}\end{matrix} \tag{8}$$

Next,

$$m_d = \frac{r_d}{2\Delta} \qquad \text{grams,} \tag{9}$$

where

$$\Delta = \frac{\omega_2 - \omega_1}{2} \qquad \text{hyps. per sec.} \tag{10}$$

[1] Bibliography 12, p. 88, and Chap. X.

ω_1 and ω_2 are the observed quadrantal angular velocities, as shown in Fig. 4.

Finally,

$$S_d = m_d\omega_o^2 \qquad \text{dynes per cm.} \qquad (11)$$

Measurement of Amplitude of Vibration, χ_{m_o}.—A special form of amplitude measurer was constructed for use with this instrument. It is illustrated in Fig. 6. *FF* is a brass frame which straddles the loud-speaker element. The feet, *ff*, of this frame are screwed firmly to the loud-speaker base. The square central pillar, *CC*, rises from the center of the frame *FF*. The upper part, *uu*, of this pillar is threaded, for receiving a screw of 40 threads to the inch. The head of this screw carries a dial, graduated 200 divisions to the circumference, over a brass index *I*, adjustable in height but fixed to the pillar. One complete revolution of the dial therefore advances the screw 0.025 inch (0.0635 cm.), and one division on the dial advances it 0.000125 inch (0.0003175 cm. = 3.175 μ). A brass spring, *S*, presses upwards upon the screw spindle. The bottom of the spindle projects through the frame and carries the brass stirrup holder, *h*, which is detachable from the spindle. The stirrup wire is a piece of fine oscillograph strip. It carries, at its center, a small triangular mirror *M*, fastened in place by shellac. The mirror rests at an angle of about 45° with the horizontal, and the apex of the triangle is directed downwards to rest upon the central screwhead of the diaphragm. The tension of the stirrup wire is adjustable by means of the screw *k*.

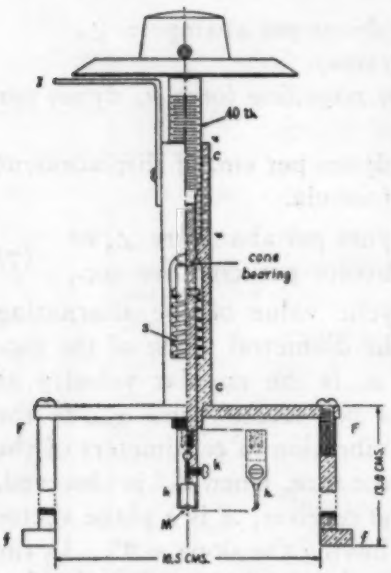

FIG. 6.—Amplitude Measurer.

A beam of light from an arc lamp is passed through a focusing lens and on to the triangular mirror, whence it is reflected to a graduated scale on a white screen.

With this apparatus, two methods may be used for measuring vibratory amplitudes. These may be called (1) the deflection method,[1] and (2) the direct method.

(1) In the deflection method, the mirror apex is applied to the diaphragm in such a manner that the mirror will follow the vibratory motion of the diaphragm faithfully, without breaking contact at any moment during the test. If it should break out of contact during vibration, the apparent deflection may be considerably greater than the true deflection, due to the shock of impact. Care must therefore be taken with the setting of the mirror to see that this condition is fulfilled. In making the measurement, the extremities of the deflection of the beam of light are marked off on the scale, making sure with the aid of a simultaneous Rayleigh bridge balance, that the diaphragm is in resonance. The testing alternating current is then removed and the same range of deflection is made on the screen with the aid of the screw S, so as to reproduce the observed deflection. The advance of the screw under these conditions will then correspond, when divided by 2, to the single amplitude of vibration at resonance, χ_{mo}.

(2) The direct method consists of slowly advancing the screw during diaphragm vibration at resonance until the mirror is just started into vibration by first contact. The testing current is then interrupted and the screw further advanced until it just makes contact with the then stationary diaphragm, as observed by the behavior of the spot of light on the screen. The advance of the screw between these two positions, in cm., is equal to the required amplitude, χ_{mo}.

The two methods were found to give results in satisfactory agreement. The resonant amplitude, χ_{mo}, was found to be in the neighborhood of 0.004 cm. or 40 microns (40 μ); or about 12 microns per rms. milliampere, the precise value depending upon the conditions of the test.

[1] Bibliography 12, p. 120.

From Fig. 4 we obtain

$\omega_o = 2\pi(633.4) = 3980$ circular radians per sec.

$$\Delta = \frac{2\pi(635.7 - 631.5)}{2} = 4.2\pi = 13.2 \text{ hyp. radians per sec.}$$

$Z_o' = 102.5 \times 10^9 = 1.025 \times 10^{11}$ abohms.

The average of A, obtained from seven independent measurements using both methods, was

$$A = 3.15 \times 10^6 \text{ dynes per abampere} \quad \text{by (7).}$$

Hence

$$r_d = 97.6 \text{ dynes per kine} \quad \text{by (8),}$$
$$m_d = 3.70 \text{ grams} \quad \text{by (9),}$$
$$S_d = 5.86 \times 10^7 \text{ dynes per cm.} \quad \text{by (10).}$$

With the lower cap B in Fig. 1, in place, the mean value of nine corresponding independent measurements gave $A = 3.22 \times 10^6$ dynes per abampere. This indicates that no considerable change in the value of A was caused by the application of the lower cap B.

In the computations for mechanic impedance using (3), A^2 was taken as 10^{13}.

Mechanic Impedance on Diaphragm under Acoustic Tube Load.—It has already been pointed out that the total mechanic impedance on the diaphragm is given by (6), and is the vector sum of three mechanic impedances; namely, that of the diaphragm z_d, that offered to the diaphragm by the lower air chamber z_1, an acoustic impedance, and that offered to the diaphragm by the upper air chamber z_2, also an acoustic impedance. We may consider these in turn.

Diaphragm Impedance, z_d, as Affected by Change of Frequency.—This has already been discussed in equation (5), and the graph of its motional admittance, Y_m, has been given in Fig. 5. The formula for converting motional admittance, Y_m, into mechanic impedance, z'', has been given in (3). It happened in these measurements, as already mentioned, that $A^2 = 10^{13}$ very nearly. Consequently

$$z'' = A^2 Y_m' = 10^{13} \times Y_m \times 10^{-9}$$
$$= Y_m \times 10^4 \text{ mechanic abohms } \angle, \qquad (12)$$

where Y_m is in mhos or reciprocal ohms. The mechanic impedance was thus equal to the motional admittance in mhos multiplied by 10^4.

In Fig. 5, the mechanic impedance scale of z_d is shown in addition to the scale of mhos. Thus, at 638.0~, $Y_m = 0.0097 + j0.0180$ mho, and $z_d = 97 + j180$ mechanic abohms. It is evident that the mechanic impedance graph of the diaphragm is a straight line parallel to the reactance axis, and 97 units from it.

It should be noted that, in accordance with (6), the mechanic impedance of the diaphragm, as obtained in the open arrangement, with both lower cap and upper tube removed, *B* and *T* in Fig. 1, includes a sum, $z_1 + z_2$, of acoustic impedances below and above the diaphragm. The diaphragm was, however, open to the air on both sides, and an approximate acoustic short circuit was thus presented. Strictly speaking, the test should have been made with the loud-speaker element in vacuo, in order to reduce z_1 and z_2 to zero for determining z_d. An attempt was made to obtain such an observation; but it was only partially successful. It appears, however, that the correction for z_1 and z_2 is not large and it is here neglected.

Lower Acoustic Impedance, z_1, with Lower Cap in Place.—The dimensions of the air space beneath the diaphragm with the lower cap in place (see Fig. 1) were not under control but were constant, and it is known that this air chamber was far from being resonant over the range of frequencies employed (600~ to 700~). This means that the acoustic impedance z_1 was a nearly constant vector over any small range of frequency, such as 10~, within these frequency limits, as will later be shown. See Appendix A.

Upper Acoustic Impedance, z_2, with a Brass Test Cylinder in Place.—When a test cylinder, *T*, is mounted over the loud-speaker element, as shown in Fig. 1, the impedance z_2 is added to $(z_d + z_1)$. The acoustic impedance, z_2, of such a tube was shown in preceding researches as being approximately similar to that of an alternating-current electric line of uniform

electrical constants.[1] The results of the present research have amplified and supported that theory.

The linear hyperbolic angle, α, and the acoustic surge impedance z_o, of such an air tube are defined by

$$\alpha = \sqrt{(r + j\omega S\rho)\left(g + j\frac{\omega}{S\gamma p_o}\right)} = \sqrt{(r + jx)(g + jb)} \text{ hyps. per cm. } \angle \tag{13}$$

and

$$z_o = \sqrt{\frac{r + j\omega S\rho}{g + j\frac{\omega}{S\gamma p_o}}} = \sqrt{\frac{r + jx}{g + jb}} \quad \text{mechanic abohms } \angle, \tag{14}$$

where S is the area of cross-section of the tube in sq. cms., γ is the ratio of specific heat at constant pressure to that at constant volume for air, taken as 1.41, ρ is the quiescent density of the air referred to 1.276×10^{-3} gm. per cc. at 0° C. under a pressure of 10^6 baryes, p_o is the quiescent pressure of the air in baryes or dynes per sq. cm., r is the linear acoustic resistance, g is the linear acoustic conductance, x is the linear acoustic reactance, and b is the linear acoustic susceptance.

The value of the linear hyperbolic angle, α, for air in this size and kind of tubing, at 660 ~ and 24° C. was found, as will appear later, to be

$$\alpha = 0.00026 + j0.1200 \qquad \text{hyps. per cm.}$$

The slope of α is obviously

$$\left(90° - \tan^{-1}\frac{.00026}{.1200}\right) = (90° - 0.124°).$$

Let

$$\tan^{-1}\frac{x}{r} = (90° - \beta_1) \tag{15a}$$

and

$$\tan^{-1}\frac{b}{g} = (90° - \beta_2), \tag{15b}$$

where both β_1 and β_2 will be positive angles.

[1] Bibliography 10 and 21.

Then α^2 will have the slope

$$[(90° - \beta_1) + (90° - \beta_2)] = [180° - (\beta_1 + \beta_2)] \quad (15c)$$

and α will have the slope

$$\left[90° - \frac{\beta_1 + \beta_2}{2}\right]. \quad (15d)$$

Also z_o^2 will have the slope

$$[(90° - \beta_1) - (90° - \beta_2)] = -(\beta_1 - \beta_2) \quad (15e)$$

and z_o will have the slope

$$-\left[\frac{\beta_1 - \beta_2}{2}\right]. \quad (15f)$$

In no case can this slope exceed in size $(\beta_1 + \beta_2)/2$. But this is the defect of the slope of α from 90°, and hence, at this frequency, the slope of z_o must be less than $\pm 0.124°$, and is therefore negligible.

The size of the angle $(\beta_1 + \beta_2)/2$ or 0.124° indicates that r is very small as compared with x, and that g is very small as compared with b. Hence z_o, by equation (14), reduces with a very good approximation to

$$z_o = \sqrt{\frac{jS\rho\omega}{j\frac{\omega}{S\gamma p_o}}} = S\sqrt{\gamma p_o \rho} \quad \text{mechanic abohms.} \quad (16)$$

From this equation, the value of z_o for this size tubing at a pressure of 10^6 baryes and a temperature of 20° C. is found to be 1170 mechanic abohms (see Appendix A).

It is now necessary to consider the fact that the amplitude of vibratory movement of the diaphragm is not uniform at all points of its surface, like that of an ordinary piston, but, as in any actual diaphragm clamped at the edge, the amplitude diminishes from the center to the edge, where it vanishes. It has been shown [1] that in the case of a steel telephone diaphragm, the acoustic impedance on its surface is less than would be developed if the amplitude were uniformly equal

[1] Bibliography 12, p. 169.

over its surface in the ratio of the equivalent mass of the diaphragm, m_d, to the actual mass, M, within the clamping circle. If we call this ratio, m_d/M, the mass factor of the diaphragm, the acoustic impedance upon the diaphragm will be reduced in this ratio.

In the case of the electrodynamic loud-speaker element used in this research, the matter is complicated by the application of the mass of the suspended coil to the center of the diaphragm, so that the ratio of diminution of the acoustic impedance, which will be denoted by a^2, is not necessarily equal to the mass factor.

Experimental means for obtaining the value of a^2 directly are not yet forthcoming, and are much to be desired. An exploration of amplitude of vibration over the surface of a corrugated diaphragm, such as was used in this research, is difficult and its accuracy doubtful. The value of the factor, a^2, has been estimated in these results by computing the surge impedance z_o, of the air column theoretically by (16), (see Appendix A), and dividing the measured, or apparent, surge impedance, $a^2 z_o$, by this value. The mean value so obtained was $a^2 = 0.204$. In other words, the apparent value of the surge impedance for the acoustic column was only 20.4 per cent. of that which would be expected over a piston diaphragm.

When a uniform air tube, AB, terminates in an acoustic impedance, σ, at B, the position angle at B is defined by

$$\tanh \delta_B = \frac{\sigma}{z_o} \qquad \text{numeric } \angle, \tag{17}$$

where z_o is the apparent surge impedance of the tube at B. If the tube terminates in a perfectly reflecting, *i.e.*, acoustically non-conducting plunger or barrier, the value of σ is infinity and hence δ_B would be $j\ \pi/2$ circular radians, or $j\underline{1}$ quadrant. Actually, no barrier possesses such properties fully, and hence $\sigma < \infty$. The position angle, δ_B, is then a small real quantity plus one quadrant imaginary, very nearly. In the case of the pistons used in these tubes, it is believed that the angle δ_B is approximately $\theta_B + j\underline{1}$, where θ_B is a small real angle caused by the loss of sound energy at the piston.

The position angle at the diaphragm end, A, is

$$\delta_A = \theta + \delta_B = L\alpha + \delta_B \qquad \text{hyps. } \angle, \tag{18}$$

where L is the tube length in cms., and α is the linear hyperbolic angle of the tube.

$$\alpha = \frac{\theta}{L} = \alpha_1 + j\alpha_2 = \alpha_1 + j\frac{\omega}{v} = \alpha_1 + j\frac{2\pi}{\lambda} \qquad \text{hyps. per cm. } \angle. \tag{19}$$

Here α_1 is the real part of α and is the attenuation constant; while α_2 is the imaginary hyperbolic angle, or a real circular angle, per cm. of tube length, expressed in radians per cm. The angular velocity ω is equal to $2\pi f$, where f is the impressed frequency. Also v is the apparent velocity of sound waves in the tube, sometimes called the group velocity. If we express α_2 in quadrants per cm.,

$$\alpha_2 = \frac{4f}{v} = \frac{4}{\lambda} \qquad \text{quadrants per cm.,} \tag{20}$$

where λ is the observed wave-length of sound in the tube.

The acoustic impedance, z_A, at the generator end of the tube is then, following the electrical analogy,

$$z_A = z_o \tanh \delta_A \qquad \text{mechanic abohms } \angle. \tag{21}$$

This is the true acoustic impedance of the air column AB, which would be offered to a diaphragm of uniform amplitude of vibration over its surface.

The acoustic impedance which the same air column offers to a diaphragm clamped around the edge, and having an amplitude reduction factor a^2, will be

$$z_A' = a^2 z_A = a^2 z_o \tanh \delta_A \qquad \text{mechanic abohms } \angle. \tag{22}$$

Hence equation (6) becomes

$$z'' = z_d + z_1 + z_2 = r_d + j\left(m_d\omega - \frac{S_d}{\omega}\right) + a^2 z_{o1} \tanh \delta_{A1} + a^2 z_{o2} \tanh \delta_{A2} \qquad \text{mechanic abohms } \angle. \tag{23}$$

It has already been pointed out that the diaphragm mechanic impedance, z_d, is constant at constant impressed frequency and testing current strength, as is also the acoustic impedance, z_1, offered by the air in the lower air chamber. Hence the only change occurring in z'' at constant frequency is that due to change in δ_{42} when the length of the upper air column is altered by shifting the plunger.

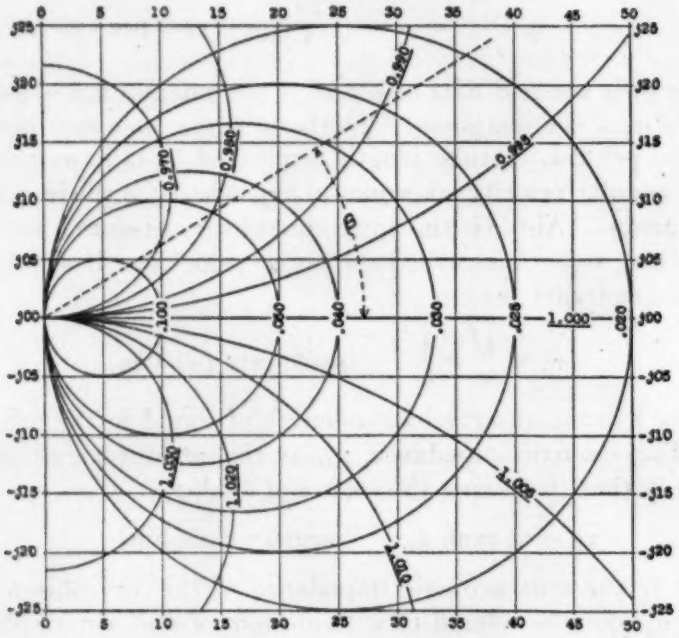

FIG. 7.—Outline chart of tanh $(x + jq)$.

Figure 7 is a small-scale reproduction of some of the curves on a chart [1] giving, by inspection, the numerical values of $\tanh \theta = \tanh (x + jq) = u + jv$, where both the hyperbolic angle θ and the tanh function are in rectangular coördinates. This particular chart covers the region.

$$\left\{\begin{matrix} x = 0.02 \text{ to } x = 0.2 \\ q = \underline{0.0} \text{ to } q = \underline{2.0} \end{matrix}\right\} \quad \left\{\begin{matrix} u = 0 \text{ to } u = 50 \\ v = -25 \text{ to } v = +25 \end{matrix}\right\}.$$

[1] Bibliography 6B, Chart IX. Third Edition.

It will be noted that the imaginary part of θ is expressed, for convenience, in quadrants, q (underlined), instead of in circular radians; since

$$\tanh \{x + j(q + 2m)\} = \tanh (x + jq), \qquad (24)$$

where m is any integer. 1 quadrant $= \pi/2$ circular radians.

The curves in Fig. 7 are all circles. It follows that if x be kept constant and q varied, $\tanh (x + jq)$ will move over the circle corresponding to the particular value of x. If x and q both increase at uniform independent rates, the locus of $\tanh (x + jq)$ will be an inwardly moving tanh spiral, approaching as a limit the point $1 + j0$, since as x approaches infinity $\tanh x$ approaches unity.

Technique for Determining the Position Angle $\delta_A = x + jq$. —If we consider the function

$$(c + j0) \tanh (x + jq) = c(u + jv)$$
$$= c\sqrt{u^2 + v^2} \angle \tan^{-1}\left(\frac{v}{u}\right) = c' \angle \beta \qquad (25)$$

it is to be observed that the final product has the same slope, β, as $\tanh (x + jq)$.

Let us suppose that we know by direct measurement the value of

$$c' \angle \beta = (c + j0) \tanh (x + jq), \qquad (26)$$

where c' and β are the only known quantities. Since β is the slope of $\tanh (x + jq)$, it follows that if, as in Fig. 7, a line be taken from the origin making an angle β with the u axis, this line must pass through the intersection of the circle corresponding to the value of x with the circle corresponding to the value of q. It follows that the value of x is determined if q is known, or the value of q is determined if x is known.

As an example, let

$$c' = 500, \beta = \tan^{-1} 0.623, q = \underline{0.990},$$

and

$$c \tanh (x + jq) = c' \angle \beta = 500 \angle \tan^{-1} 0.623.$$

This is the case represented in Fig. 7, where β is $\tan^{-1} 0.623$ or 31.9°. The intersection of the dotted line with the circle

of $q = \underline{0.990}$ determines a point $u + jv = 28.7 + j\,17.9$ and the value of the x circle passing through this point is $x = 0.025$ Hence

$$x + jq = 0.025 + j\,\underline{0.990}, \quad \text{and} \quad c = \frac{c' \angle \beta}{\tanh (x + jq)}$$

$$= \frac{500 \angle \beta}{(28.7)^2 + (17.9)^2 \angle \beta} = \frac{500}{33.8} = 14.8.$$

Thus it is evident that a knowledge of β and q suffices to determine x. If c' also is known, c is then determined. This is the method which has been employed throughout, for evaluating acoustic impedances of the type $a^2 z_0 \tanh \delta_A$.

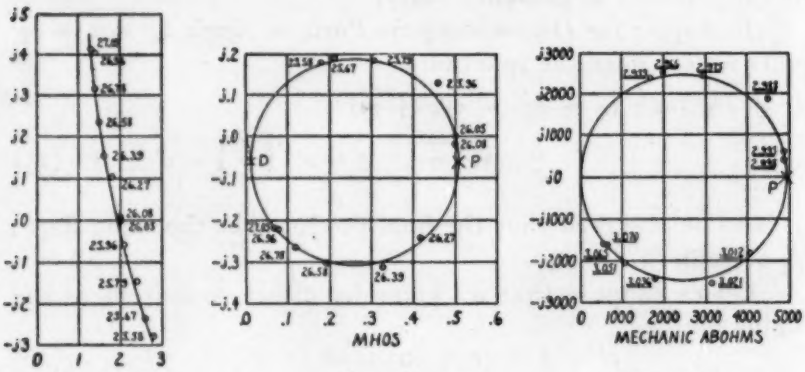

FIG. 8A FIG. 8B FIG. 8C

FIG. 8.—(*A*) Motional impedance, Z_m; (*B*) motional admittance, Y_m; (*C*) mechanic impedance, z_1 due to upper tube.
Piston type T_1, $F = 658.7\sim$, $T = 24.7°$ C. at half-wave resonance, $\delta_B = 0.0177 + j\,\underline{1}$, $\delta_A = 0.0245 + j\,\underline{3}$.

Figure 8 shows at *A*, *B*, and *C*, respectively, the observed motional impedance, motional admittance, and mechanical impedance, due to the upper tube, for the loud-speaker element of Fig. 1, with tube and plunger type T_1 mounted over it and the tube length varied from 25.58 to 27.03 cms, at the constant impressed frequency of $658.7\sim$, and a temperature of 24.7° C. It will be seen that the motional impedance is nearly a circular locus. The inversion of Fig. 8*A* gives rise to the nearly circular locus of Fig. 8*B*, which is the motional

admittance Y_m, of the system, under these conditions. When multiplied by $A^2 \times 10^{-9}$ the points on this figure are converted to units of mechanical impedance, according to (3). The circle of Fig. *8B* is displaced by the vector $OD = .013 - j\,.060$ and consequently, by (3), (12), and (23), the constant mechanic impedance $(z_d + z_1)$ of the diaphragm and lower air chamber, for these conditions, was $130 - j\,600$ mechanic abohms. In Fig. 8*C*, the conversion to mechanical units has been made by (12), and the constant vector $z_d + z_1 = 130 - j\,600$ subtracted from each point. Hence Fig. 8*C* gives the upper mechanic impedance, z_2, due to the upper air chamber alone. It has been assumed that the acoustic surge impedance z_o has substantially zero slope, as already indicated (15*h*). This has been borne out by the measurements up to this time; although it may need future correction. In the case of these particular tubes, however, the slope of z_o must be very small. Consequently, the slope of $a^2 z_{o2} \tanh \delta_{A2}$ must be the same as the slope of $\tanh \delta_{A2}$ itself.

In the case of the plungers T_1 and T_2, it appears that their mechanic impedance σ, is a large resistance; so that δ_{B2}, from (17), is substantially equal to $\theta_B + j\underline{1}$, or a small real hyperbolic angle plus one quadrant imaginary. Consequently, when the angle of the tube is $\theta_1 + j\underline{2m}$; or has $2m$ quadrants as its imaginary component, m being any positive integer including zero, δ_A becomes

$$\delta_A = (\theta_1 + \theta_B) + j(\underline{2m + 1}) \qquad \text{hyps. } \angle. \qquad (27)$$

At any such point, $\tanh \delta_A = x + j0$, a pure real quantity. Multiplying this by $a^2 z_o$, itself without slope, leaves $a^2 z_o \tanh \delta_A$ still a pure real quantity. Hence the point P, at the end of the diameter OP, Fig. 8*C*, corresponds to $2m + 1$ quadrants in δ_A. The velocity of sound in air is known to be (33120 + 60 T) cm. per sec., where T is the temperature in deg. C.; or in this case $V = 34660$ cm. per sec. It is also known that the velocity of plane sound waves in a tube does not fall greatly below this value. Hence, the wave-length of sound waves in free air at this frequency, 658.7 ~, should be very

nearly 34660/658.7 or 52.6 cm., and a half-wave-length 26.3 cm. This is close to the length of the air column at point P in Fig. 8C. Hence, the length of the air column must have corresponded to two quadrants at this point. Adding one quadrant due to δ_B gives, at P, $\delta_A = \theta_1 + \theta_B + j\underline{3}$. It appears in Fig. 8$B$, that the tube length at P was 26.11 cm. and this corresponds to two quadrants. Thus, one quadrant at this frequency and temperature corresponded to 13.06 cm. and one cm. of tube to $\underline{0.0766}$ quadrant. The circular quadrant measure, Q_p, can thus be assigned to any observed point, p, around the circle, as has been done in Fig. 8C.

In general, if n is the number of wave-lengths in a tube at a diametral point, such as P in Fig. 8C, then

$$Q_o = 4n \qquad \text{quadrants,} \tag{28}$$

where Q_o is the number of quadrants at the diametral point. If the length, L_o, of the air column is known at this point, the length corresponding to one quadrant will be $L_o/4n$. Hence the quadrantal measure, Q_p, at any point, p, is

$$Q_p = \frac{L_p}{\frac{L_o}{4n}} = \frac{4nL_p}{L_o} \qquad \text{quadrants,} \tag{29}$$

where L_p is the length of the air column at the point p. This enables the method of Fig. 7 to be employed for finding the real component, $\theta_1 + \theta_B$, of δ_A and also the value of a^2z_o.

Experimental Results with Air Tube and Plunger T_1

Figure 8C shows that the locus of acoustic impedance on the diaphragm due to the upper tube with the length of upper air column varied between 25.58 and 27.03 cm. is substantially a circle. This would be expected because the real part, $\theta_1 + \theta_B$, of δ_A is so nearly constant over such a small change in L. If, however, the measurement were to be extended over a range of many wave-lengths in L, we should expect the locus of Fig. 8C to become an inwardly directed spiral, with a point at a^2z_o as the pole. This will be indicated in the results which are to be described.

A slight uncertainty exists as to the air column lengths L, owing to the corrugation of the diaphragm (Fig. 1) which is not a plane surface. The extent of this uncertainty is ± 0.8 mm. Mean values are given.

The method of Fig. 7 gives an average value of $\theta_1 + \theta_B = 0.0245$, for the points shown in Fig. 8*C*.

Results with Plunger T_2.—Plunger T_2 fitted the tube more closely than T_1, and the packing was also brought closer up to the reflecting metallic surface, so as to diminish the depth of any small crevice around the edge of the piston. Fig. 9 shows

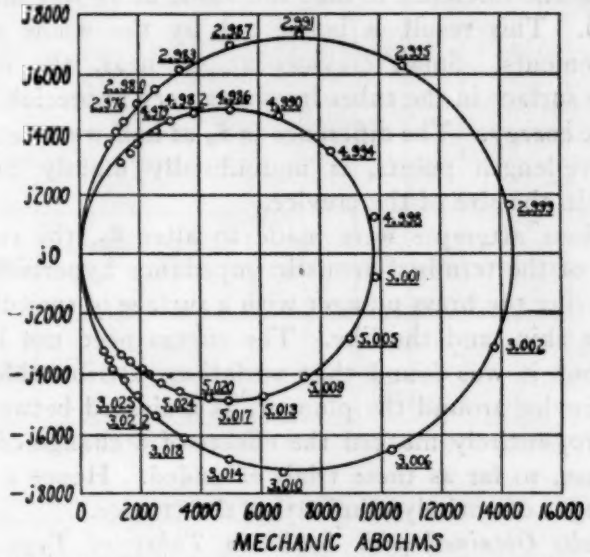

FIG. 9.—Mechanic impedance, z_2, due to upper tube. Piston type T_2, $F = 658.7\sim$ $T = 24.2°$ C.

At half-wave resonance $\delta_B = 0.0082 + j\underline{1}$, $\delta_A = 0.0150 + j\underline{3}$
At one-half resonance $\delta_B = 0.0119 + j\underline{1}$, $\delta_A = 0.0255 + j\underline{5}$

the mechanic impedance due to the upper tube, z_2, omitting the steps by which it is derived from the motional impedance (see Figs. 8*A*, 8*B*, and 8*C*). It consists of a spiral which appears as practically two circular loops. The outer circle was obtained by varying the length of air column, L_2, about the half-wave length point. The inner loop was similarly ob-

tained by varying L_2 about the one-wave-length point. The lengths expressed in quadrants, Q_p, according to (29) are marked around the loops.

When vaseline was applied more liberally to the lower edge of the plunger, T_2, so as to fill up, as far as possible, any air crevice that might form there, a distinct effect was found in the measurements, which are presented in Fig. 10. The mechanic impedance loops, especially the outer half-wave-length loop, are larger, indicating a reduced value of $\theta_1 + \theta_B$. As there was no reason to suppose that θ_1 was in any way affected, the inference is that the value of θ_B was materially reduced. This result is borne out by the whole series of measurements. Small crevices at, or near, the reflecting plunger surface in the tubes involve a very appreciable loss of acoustic energy. The difference in θ_B at half-wave length and one-wave length points, is undoubtedly mainly due to a change in the size of the crevice.

Various attempts were made to alter θ_B, the real component of the terminal acoustic impedance hyperbolic angle, by covering the brass plunger with a surface of smooth paper, chamois skin, and the like. The curves need not be given here; but it was found that variations attributable to the small crevice around the plunger, as indicated between Figs. 9 and 10, entirely masked the effects of a change of surface substance, so far as these trials extended. Hence a method was sought of entirely eliminating the crevice.

Results Obtained with Sealed-in Tubes of Type T_3.—In order to eliminate the crevice around the piston completely, the tube was sealed, by soldering a flat brass plate, of the same thickness as the tubing, over the upper end and then cutting the lower end of the tube, in a lathe, to the desired length.

The first procedure attempted was successively cutting off in the lathe small lengths of about 1 cm., in steps of 1 mm. or less, and measuring the motional impedance for each stage, the frequency being kept constant at that value which would produce half-wave resonance near the mean length. This was

found to be a very tedious and unsatisfactory process. The change in motional admittance due to cutting off 1 mm. from the tube, near a half-wave resonant point, was very noticeable; but it was complicated by temperature changes, due to the

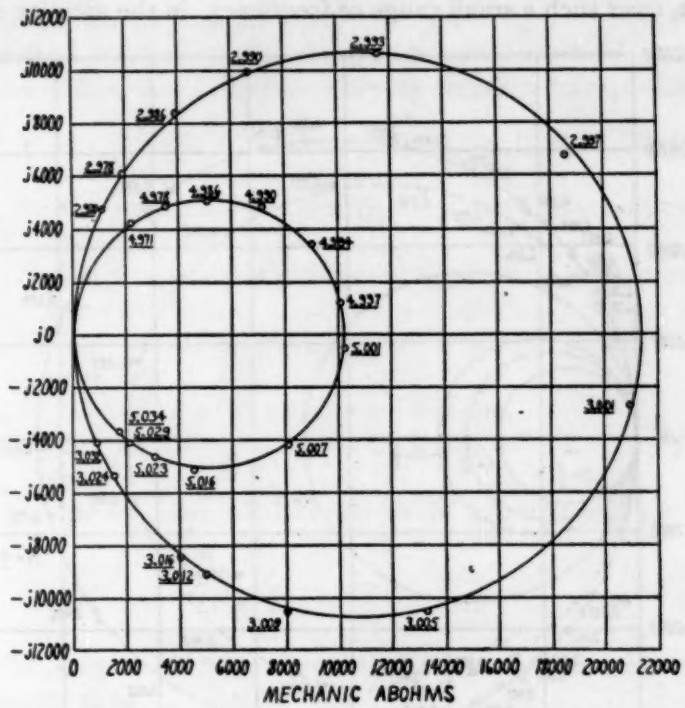

FIG. 10.—Mechanic impedance z_2, due to upper tube. Piston type T_2 well vaselined. $F = 660.1\sim$, $T = 23.1°$ C.

At half-wave resonance $\delta_B = 0.0039 + j\,1$, $\delta_A = 0.0107 + j\,3$
At one-wave resonance, $\delta_B = 0.0103 + j\,1$, $\delta_A = .0239 + j\,5$

heat generated in the tube by cutting. Much delay was involved in waiting for the freshly cut tube to reach room temperature. In each case, the seal at *JJ* in Fig. 1, had to be made with great care. This procedure was therefore abandoned, in favor of a method employing constant sealed-in tube length near a half-wave-length point at the desired frequency, and then varying the impressed frequency slightly ($\pm 5\sim$), so as to develop the motional-admittance diagram.

It is shown in Appendix A, that a change in impressed frequency introduces corresponding changes, not only in z_2 but also in z_d and z_1. The change in z_d is definitely known, and the results are easily corrected for this change. The change in z_1 over such a small range of frequency, in the vicinity of the

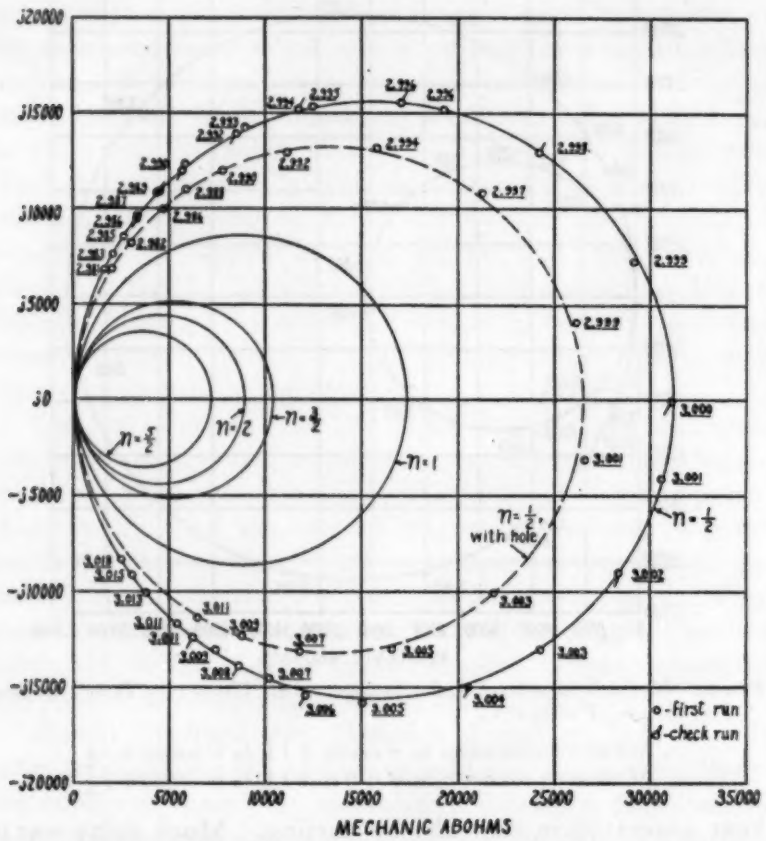

FIG. 11*A*.—Mechanic impedance, z_2, offered by different lengths of sealed-end tube over frequency range at resonance of 658.2∼ to 661.9∼ and a temperature range of 22.7 to 24.9° C.

At $n = \frac{1}{2}$, $L =$	26.08 cms.,	$\delta_B = 0.0007 + j\,1$,	$\delta_A = 0.0075 + j\,3$
1	52.28	$0.0010 + j\,1$	$0.0146 + j\,5$
$\frac{3}{2}$	78.49	$0.0020 + j\,1$	$0.0224 + j\,7$
2	104.66	$0.0007 + j\,1$	$0.0281 + j\,9$
$\frac{5}{2}$	130.86	$0.0010 + j\,1$	$0.0348 + j\,11$
$\frac{1}{2}$, hole	26.08	$0.0033 + j\,0.992$	$0.0101 + j\,2.992$

test frequency employed, is fortunately negligibly small. *The art of measuring acoustic impedance, z_2, under varying impressed frequency, thus consists in making such adjustments that z_1 shall vary but slightly, while z_2 varies considerably, and z_3 varies in a known manner in formula* (6).

Method of Finding Q_p, the Quadrantal Length of an Air Column of Constant Length, at Varying Impressed Frequency.—

Let v be the velocity of sound waves in the tube at the temperature T, in cm. per sec.,

f_p be the impressed frequency at a given observation in cycles per sec.,

f_o be the frequency which gives upper tube resonance at some multiple of a half-wave-length in cycles per sec.,

n be the number of wave-lengths at which f_o gives resonance (1/2, 1, 3/2, . . .),

L_2 be the length of the upper tube in cm.,

λ_o be the wave-length of sound waves at frequency f_o in cm.,

λ_p be the wave-length of sound waves at frequency f_p in cm.

Then

$$L_2 = n\lambda_o = \frac{nv}{f_o} \quad \text{cm.} \tag{30}$$

The length of tube corresponding to one quadrant at frequency f_p is

$$\frac{\lambda_p}{4} = \frac{v}{4f_p} \quad \text{cm.} \tag{31}$$

The number of quadrants, Q_p, at any observation with impressed frequency f_p is then

$$Q_p = \frac{\frac{nv}{f_o}}{\frac{v}{4f_p}} = \frac{4nf_p}{f_o} \quad \text{quadrants.} \tag{32}$$

Hence

$$\delta_A = (\theta_1 + jQ_p) + (\theta_B + j1) \quad \text{hyps. } \angle. \tag{33}$$

Results Obtained with Variable-Frequency Method on Sealed-End Tubes, Type T3.—Figs. 11*A* and 11*B* show the acoustic

impedance, z_2, offered to the diaphragm by the upper air column in sealed-end brass tubes of lengths resonating at $n = 1/2, 1, 3/2, 2$, and $5/2$ wave-lengths at a mean frequency of about 660 ~. These different air columns were obtained from

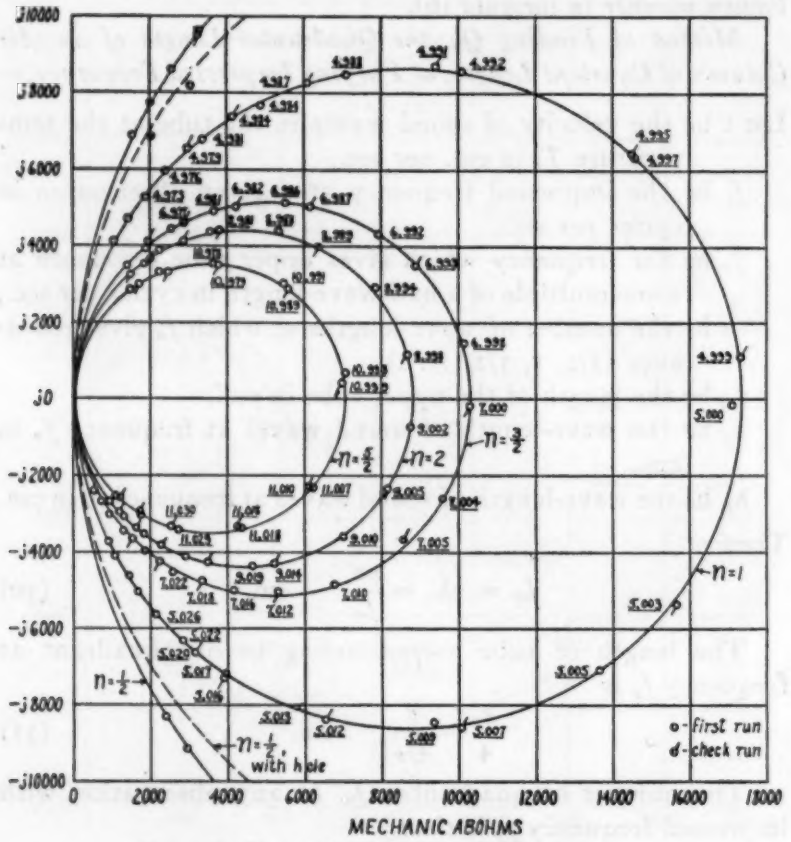

FIG. 11*B*.—Part of Fig. 11*A* drawn to larger scale.

different lengths of the same size of brass tubing. The method by which the results were computed from observed data, is shown in Appendix B.

These five circles are five successive loops of a single diminishing spiral, corresponding to equation (22). The outer circle corresponds to measurements near a half-wave length, and for it $\theta_1 + \theta_R = 0.0075$. Also $a^2 z_0 = 233$. The two

series of observations, taken several days apart, from which this circle has been obtained, are marked differently in Fig. 11*A*, and are in satisfactory agreement. Comparing this circle with the corresponding half-wave length circles of Figs. 9 and 10, it is evident that the real part of the hyperbolic angle $\theta_1 + \theta_B$, is distinctly less for the sealed-end tube. Since θ_1 must have been substantially the same in all three cases, the difference must be attributed to θ_B. In other words, the minute crevice formed by a fairly tight and packed plunger, in the cases of Figs. 9 and 10, was responsible for a very appreciable acoustic loss, as indicated by the values of θ_B.

The tubes used were as follows:

(*a*)	A sealed or soldered-end tube,	24.70 cm. long
(*b*)	A sealed or soldered-end tube,	50.90 cm. long
(*c*)	A sealed or soldered-end tube,	77.11 cm. long
(*d*)	An open section of the same.	78.58 cm. long

The effective length of the stationary tube, soldered to and a part of the upper clamping ring, C_2 in Fig. 1, was taken as 1.38 cm. From the above sections, the following effective tube lengths were made up:

(1)	26.08 cm. from (*a*);	n = 1/2 wave-length
(2)	52.28 cm. from (*b*);	n = 1 wave-length
(3)	78.49 cm. from (*c*);	n = 3/2 wave-lengths
(4)	104.66 cm. from (*a*) and (*d*);	n = 2 wave-lengths
(5)	130.86 cm. from (*b*) and (*d*);	n = 5/2 wave-lengths

It will be seen that Nos. (1) and (4) had the same terminal load, or seal construction; while Nos. (2) and (5) also have the same terminal load. Hence δ_B must be the same in (1) as in (4) and likewise the same in (2) as in (5). Using these pairs we obtain the following results:

$$\theta_{104.66} + \theta_{B_{1 \text{ and } 4}} = 0.0281 \quad + j\,\underline{9}$$

$$\theta_{26.08} + \theta_{B_{1 \text{ and } 4}} = 0.0075 \quad + j\,\underline{3}$$

Hence,

$$\theta_{78.58} \cdot \cdot \cdot \cdot = 0.0206 \quad + j\,\underline{6}$$

or, dividing by 78.58,

$$\alpha \cdot \cdot \cdot \cdot \cdot \cdot = 0.000262 + j\,\underline{0.07636}$$

Again,

$$\theta_{130.86} + \theta_{B_2 \text{ and } 5} = 0.0348 \quad + j\,\underline{11}$$
$$\theta_{52.28} + \theta_{B_2 \text{ and } 5} = 0.0146 \quad + j\,\underline{5}$$

Hence,

$$\theta_{78.58} \cdot \cdot \cdot \cdot \cdot \cdot = 0.0202 \quad + j\,\underline{6}$$

or, dividing by 78.58,

$$\alpha \cdot \cdot \cdot \cdot \cdot \cdot \cdot = 0.000257 + j\,\underline{0.07636}$$

The mean value of α from these two sets of observations was

$$\alpha = 0.000260 + j\,\underline{0.07636} \quad \text{hyp. per cm.}$$

The length of straight, indefinitely long brass tube of this type, in which the amplitude of sound vibration would be reduced to $1/\epsilon$, or 36.8 per cent. would be 1/0.00026, or 3845 cm. This may be called the *exponential length.*

In Fig. 11, the dotted circle shows the result obtained when a No. 60 drill hole was bored through the center of the cap of tube (*a*). The diameter of this hole is 0.040 in., or approximately 1 mm.

The following Table I. gives the analysis of all the results here reported:

It should be noted that in all of these series α_1 has been taken as constant at the mean value of 0.000260 hyp. per cm. for the mean frequency of 660~. It is believed, however, from experimental evidence not here reported and from the form of equation (13), that the value of α_1 increases slightly with frequency, as is known to be the case in alternating-current electric lines.

The last column of the table gives the values of a^2z_o, the apparent surge impedance of the air column on the upper surface of the diaphragm. If the amplitude of vibration were the same over the entire surface, like that of a piston, a^2 would be unity. The mean value of a^2z_o is 239 mechanic abohms.

TABLE I

COLLECTION OF RESULTS

Fig.	Type of End	Tubes	L_2 cm.	n	δ_A hyps. ∠	θ hyps. ∠	δ_B hyps. ∠	α_1 hyp./cm.	$a^2\rho_0$ Mech. Abohms
8	T_1	—	26.11	1/2	0.0245 $+j$ 3	0.0068 $+j$ 2	0.0177 $+j$ 1	0.00026	210
9	T_2	—	26.16	1/2	0.0150 $+j$ 3	0.0068 $+j$ 2	0.0082 $+j$ 1	0.00026	213
9	T_3	—	52.32	1	0.0255 $+j$ 5	0.0136 $+j$ 4	0.0119 $+j$ 1	0.00026	250
10	T_2	—	26.21	1/2	0.0107 $+j$ 3	0.0068 $+j$ 2	0.0039 $+j$ 1	0.00026	229
10	T_3	—	52.40	1	0.0239 $+j$ 5	0.0136 $+j$ 4	0.0103 $+j$ 1	0.00026	244
11	Seal	(*a*)	26.08	1/2	0.0075 $+j$ 3	0.0068 $+j$ 2	0.0007 $+j$ 1	0.00026	233
11	Seal	(*b*)	52.28	1	0.0146 $+j$ 5	0.0136 $+j$ 4	0.0010 $+j$ 1	0.00026	254
11	Seal	(*c*)	78.49	3/2	0.0224 $+j$ 7	0.0204 $+j$ 6	0.0020 $+j$ 1	0.00026	230
11	Seal	(*d*) + (*a*)	104.66	2	0.0281 $+j$ 9	0.0274 $+j$ 8	0.0007 $+j$ 1	0.00026	248
11	Seal	(*d*) + (*b*)	130.86	5/2	0.0348 $+j$ 11	0.0338 $+j$ 10	0.0010 $+j$ 1	0.00026	246
11	Hole	(*a*)	26.08	1/2	0.0101 $+j$ 3	0.0068 $+j$ 2.0076	0.0033 $+j$ 0.9924	0.00026	271

NOTE. The j numerals are all expressed in quadrants.

Summary of Conclusions

1. The acoustic impedance of a short uniform air-tube is closely analogous to that of a uniform alternating-current electric line.

2. The attenuation constant of the brass air tube used was found to be 0.00026 hyp. per cm. at 660 ~ and 24° C. or the exponential length 38.45 meters.

3. The terminal load impedance of a closed acoustic tube is similar to that of an a.–c. electric line terminating in a high resistance.

4. The effect of a small crevice around the edge of a plunger closing the tube, was very noticeable upon the measured value of the terminal acoustic load.

5. By soldering the cap to the tube, so as to eliminate this crevice, the apparent angle of the terminal load was reduced to $0.0007 + j\,\underline{1}$ in the most successful case.

6. Drilling a 1-mm. hole through the center of the terminal cap (1.6 mm. thick), produced a very noticeable effect upon the terminal load angle, changing it from $0.0007 + j\,\underline{1}$ to $0.0033 + j\,\underline{0.9924}$ hyp. Taking a^2z_0 as 239, $\angle$ 0°, this means that the drill hole reduced the acoustic impedance σ, from $340000 + j\,0$ to $5200 + j\,18600$ mechanic abohms.

Appendix A

Calculation of z″, z_d, z_1, and z_2 from Assumed Constants.— (1) Calculation of z_d by equation (5), $z_d = r_d + j(m_d\omega - S_d/\omega)$

TABLE II

Values of Diaphragm Impedance z_d over Frequency Range 594 to 726 ~

f_p ~	r_d Mech. Abohms	$m_d\omega$ Mech. Abohms	$\frac{S_d}{\omega}$ Mech. Abohms	$m_d\omega - \frac{S_d}{\omega}$ Mech. Abohms	z_d Mech. Abohms
594	97	13,809	15,701	−1,892	$97 - j\,1,892$
650	97	15,111	14,349	762	$97 + j\,762$
658.68	97	15,312	14,160	1,152	$97 + j\,1,152$
660	97	15,343	14,132	1,211	$97 + j\,1,211$
661.32	97	15,374	14,103	1,271	$97 + j\,1,271$
670	97	15,575	13,921	1,654	$97 + j\,1,654$
700	97	16,273	13,324	2,949	$97 + j\,2,949$
726	97	17,435	12,436	4,999	$97 + j\,4,999$

Taking r_d = 97 dynes per cm. per sec.,

m_d = 3.70 gms.,

S_d = 5.86×10^7 dynes per cm.

(2) Calculation of z_1 by equation (22), $z_1 = a^2 z_o \tanh \delta_A$.

Taking L = 3.5 cm., effective,

S = 57.2 sq. cm., effective,

T = 20° C.,

v = 34320 cm. per sec.,

p_o = 10^6 baryes,

γ = 1.41,

ρ = 1.189×10^{-3} gm. per cc.,

α_1 = .001 hyp. per cm.,

δ_B = $.00350 + j\,1$ hyp.,

a^2 = 0.2.

By equation (16), $z_o = S\sqrt{\gamma p_o \rho}$,

$$z_o = 57.2\sqrt{1.189 \times 10^{-3} \times 1.41 \times 10^6} = 2{,}340 \text{ mech. abohms.},$$

$a^2 z_o$ = 468 mech. abohms.

By equation (18),

$$\delta_A = L\alpha + \delta_B,$$
$$\delta_A = L\alpha_1 + jL\alpha_2 + \delta_B,$$
$$\delta_A = 3.5(.001) + .00350 + j(Q_p + \underline{1}),$$
$$\delta_A = 0.0070 + j(Q_p + \underline{1}).$$

By equation (31), the length of air column corresponding to one quadrant is $v/4f$.

Hence,

$$Q_p = \frac{L}{\frac{v}{4f_p}} = \frac{4f_p L}{v} = \frac{4 \times 3.5 f_p}{34320} = \frac{7f_p}{17160}.$$

TABLE III

COMPUTATION OF LOWER ACOUSTIC IMPEDANCE z_1 OVER FREQUENCY RANGE 594 TO 726~

f_p ~	Q_p Quadrants	δ_A Hyps.	tanh δ_A Numeric	$z_1 = a^2 z_0 \tanh \delta_A$ Mech. Abohms
594	.2423	$.0070 + j\ 1.2423$	$.05 - j\ 2.50$	$23 - j\ 1172$
650	.2651	$.0070 + j\ 1.2651$	$.04 - j\ 2.27$	$19 - j\ 1063$
658.68	.2687	$.0070 + j\ 1.2687$	$.04 - j\ 2.24$	$19 - j\ 1050$
660	.2692	$.0070 + j\ 1.2692$	$.04 - j\ 2.235$	$19 - j\ 1047$
661.32	.2698	$.0070 + j\ 1.2698$	$.04 - j\ 2.23$	$19 - j\ 1044$
670	.2733	$.0070 + j\ 1.2733$	$.04 - j\ 2.20$	$19 - j\ 1030$
700	.2855	$.0070 + j\ 1.2855$	$.03 - j\ 2.09$	$19 - j\ 978$
726	.3050	$.0070 + j\ 1.3050$	$.03 - j\ 1.92$	$14 - j\ 898$

(3) Calculation of z_2 by equation (22), $z_2 = a^2 z_0 \tanh \delta_A$

Taking $L =$ 130.0 cm.,
$S =$ 28.6 sq. cm.,
$T =$ 20° C.,
$v =$ 34320 cm. per sec.,
$p_0 = 10^6$ baryes,
$\gamma =$ 1.41,
$\rho = 1.189 \times 10^{-3}$ gm. per cc.,
$\alpha_1 =$ .00025 hyp. per cm.,
$\delta_B = .00350 + j\ \underline{1}$ hyp.,
$a^2 =$ 0.2.

By equation (16), $z_0 = S\sqrt{\gamma p_0 \rho}$

$$z_0 = 28.6\sqrt{1.41 \times 10^6 \times 1.89 \times 10^{-3}} = 1170$$

mech. abohms,

$a^2 z_0 = 234$ mech. abohms.

By equation (18),

$$\delta_A = L\alpha + \delta_B,$$
$$\delta_A = 130(.00025) + .00350 + j(Q_p + \underline{1}),$$
$$\delta_A = .03600 + j(Q_p + \underline{1}).$$

As for the case of z_1,

$$Q_p = \frac{4 f_p L}{v},$$

$$Q_p = \frac{4 \times 130 \times f_p}{34320} = \frac{f_p}{66}.$$

TABLE IV

COMPUTATION OF UPPER ACOUSTIC IMPEDANCE z_2 OVER FREQUENCY RANGE 594 TO 726~

f_p ~	Q_p Quadrants	δ_A Hyps.	$\tanh \delta_A$ Numeric	$z_2 = a^2 z_0 \tanh \delta_A$ Mech. Abohms
594	9.0000	$.0360 + j\,10.0000$	$.0360 + j\,0$	$8 + j\,0$
650	9.8485	$.0360 + j\,10.8485$	$.65 + j\,4.03$	$152 + j\,943$
658.68	9.9800	$.0360 + j\,10.9800$	$15.8 + j\,13.7$	$3700 + j\,3206$
660	10.0000	$.0360 + j\,11.0000$	$27.78 + j\,0$	$6490 + j\,0$
661.32	10.0200	$.0360 + j\,11.0200$	$15.8 - j\,13.7$	$3700 - j\,3206$
670	10.1515	$.0360 + j\,11.1515$	$.65 - j\,4.03$	$152 - j\,943$
700	10.6061	$.0360 + j\,11.6061$	$.055 - j\,.715$	$13 - j\,167$
726	11.0000	$.0360 + j\,12.0000$	$.0360 + j\,0$	$8 + j\,0$

(4) Calculation of z'' by equation (23),

$$z'' = z_d + z_1 + z_2.$$

TABLE V

COMPUTATION OF TOTAL MECHANIC IMPEDANCE z'' OF VIBRATORY SYSTEM

f_p ~	z_d Mech. Abohms	z_1 Mech. Abohms	z_2 Mech. Abohms	z'' Mech. Abohms
594	$97 - j\,1892$	$23 - j\,1172$	$8 + j\,0$	$128 - j\,3064$
650	$97 + j\,762$	$19 - j\,1063$	$152 + j\,943$	$268 - j\,542$
658.68	$97 + j\,1152$	$19 - j\,1050$	$3700 + j\,3206$	$3816 + j\,3308$
660	$97 + j\,1211$	$19 - j\,1047$	$6490 + j\,0$	$6606 + j\,164$
661.32	$97 + j\,1271$	$19 - j\,1044$	$3700 - j\,3206$	$3816 - j\,2979$
670	$97 + j\,1654$	$19 - j\,1030$	$152 - j\,943$	$268 - j\,319$
700	$97 + j\,2949$	$19 - j\,978$	$13 - j\,167$	$124 + j\,1804$
726	$97 + j\,4999$	$14 - j\,898$	$8 + j\,0$	$119 + j\,4101$

These results are shown in Figs. 12*A* and 12*B*. It will be noted that the values of z_d here obtained are the actual values for this particular diaphragm. The constants assumed for the calculation of z_1 are such that the calculated value at

660~ checks the observed value. It may be seen how small is the change in z_1, when the frequency is varied from 650~ to 670~. It is apparent that only a very small error results from assuming that z_1 is constant over a frequency range of 10~,

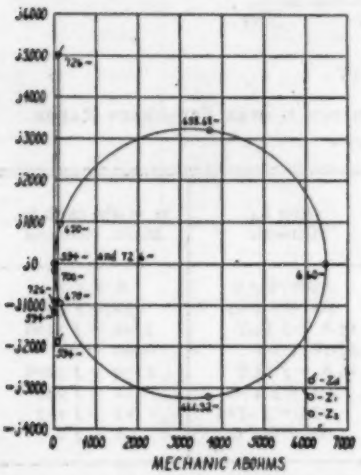

FIG. 12*A*.—Values of z_d, z_1, and z_2 calculated from assumed acoustic constants.

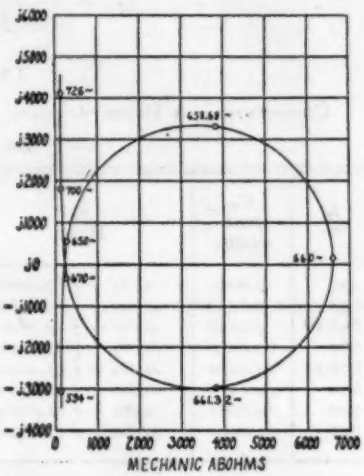

FIG. 12*B*.—Value of z'' calculated from assumed acoustic constants.

near 660~. Therefore, if the observed values of z'' are corrected for the change in z_d, for points within such a small frequency range, the resulting curve will be the locus of z_2, plus a constant vector representing the mean value of $z_d + z_1$ for this frequency. By subtracting this constant vector, $z_d + z_1$, the values of z_2 are obtained. This is the method employed for evaluating z_2 from the observed data with sealed-end tubes, as shown in Appendix B.

APPENDIX B

Method of Calculating Results from Observed Data for Constant Tube Length—Varying Frequency Runs.—As an example illustrating the method, the computations for the case of a sealed-end upper tube with an effective length of 104.66 cm. will be shown. The results appear in Fig. 11*B* as the check run for $n = 2$. The field current was maintained at 0.500

amp. and the current in the moving coil at 3.8 m.a. for these measurements. The temperature of the air was 24.8° C. A ten-to-one bridge ratio was used throughout, and hence the values in columns (3) and (4) are ten times the values of the resistance and inductance of the moving coil of the vibratory system.

The observed data appear in columns (1), (3), and (4). The values in column (2) are obtained from the oscillator calibration. Over this small range of frequency, the damped resistance, R_d, and damped inductance, L_d, are practically constant and hence one observation at a mean frequence is sufficient.

The first step in the computation is to find the values of L_m, the motional inductance, for each point, as shown in column (5). $L_m = L_f - L_d$. From these values it is easy to compute X_m for each point, and hence Z_m is obtained by equation (1), as shown in column (6). Y_m is obtained by the inversion of Z_m, according to equation (2) and shown in column (7). z'' is then obtained by equation (12) and shown in column (8).

The next step is to plot z'', the results shown in column (8), in rectangular coördinates and with compasses pass a circle as closely as possible through these points. This is the circle *a* in Fig. 13. Through the center of this circle, *b*, draw a diameter *dd* parallel to the resistance axis. The resonant frequency, f_o, is determined by the intersection of the circle with this diameter. In this case, the value of f_o is 660.3 ~.

It is now possible to compute the correction due to the change in z_d over this frequency range.

$$z_d = r_d + j\left(m_d\omega - \frac{S_d}{\omega}\right), \quad \text{mech. abohms}$$

$$z_d = r_d + j\left(2\pi f m_d - \frac{S_d}{2\pi f}\right), \quad \text{mech. abohms}$$

$$\frac{dz_d}{df} = j\left(2\pi m_d + \frac{S_d}{2\pi f^2}\right). \quad \text{mech. abohms per cyp.}$$

At $f = 660\sim$, substituting values,

$$\frac{dz_d}{df} = j(2\pi \times 3.70) + j\left(\frac{5.86 \times 10^7}{2\pi \times [660]^2}\right)$$
$$= j(23.3 + 21.4)$$
$$= j44.7 \qquad \text{mech. abohms per } \sim.$$

The value of this correction is obtained for each point by multiplying this result by the difference in frequency, $f_p - f_o$, column (9). The amount of the correction is shown in column (10) and the corrected values of z'' in column (11).

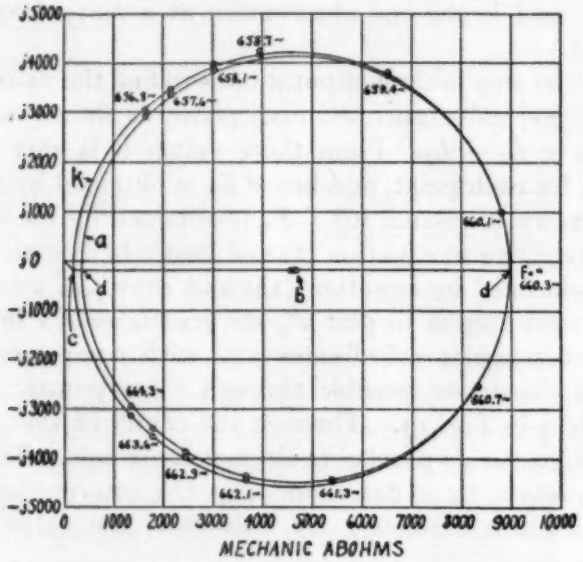

FIG. 13.—Steps in the computation of z_d from constant tube length-varying frequency data for the case where $L =$ 104.66 cm. and $T = 24.8°$ C. This is the check run for $n = 2$ in fig. 11*B*.

It is now possible to find the value of Q_p for each point according to equation (32). As the oscillator calibration was computed with the aid of five place logarithms, it was most convenient to perform this computation with the values of log f_p then obtained, column (12). log f_o was taken as 2.81974 in obtaining the results in column (13). The values of Q_p are not accurate to more than four significant figures, and even then the last figure is doubtful. Nevertheless the

TABLE VI

TABULATED DATA AND COMPUTATIONS

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Oscillator Setting	f_p	R in C arm of bridge ohms	L in C arm of bridge millihenries	L_m millihenries	$Z_m = R_m + jX_m$ ohms	$Y_m = G_m + jB_m$ mhos	z'' mech. abohms	$f_p - f_0$	Correction for change in z_d mech. abohms	Corrected z'' mech. abohms
Damped Impedance Measurement										
5-1-1	660.1	82.9	9.2							
Free Impedance Measurement										
5-1-6	656.8	97.7	2.85	− .635	$1.48 - j2.62$	$.164 + j.290$	$1{,}640 + j2900$	−3.5	$j160$	$1{,}640 + j3060$
5-1-5	657.6	96.5	4.1	−.51	$1.36 - j2.11$	$.216 + j.335$	$2{,}160 + j3350$	−2.7	$j120$	$2{,}160 + j3470$
5-1-4	658.1	95.5	5.3	−.39	$1.26 - j1.61$	$.302 + j.385$	$3{,}020 + j3850$	−2.2	$j100$	$3{,}020 + j3950$
5-1-3	658.7	94.9	6.15	−.305	$1.20 - j1.26$	$.396 + j.415$	$3{,}960 + j4150$	−1.6	$j\ 70$	$3{,}960 + j4230$
5-1-2	659.4	94.4	7.6	−.16	$1.15 - j\ .66$	$.653 + j.376$	$6{,}530 + j3760$	− .9	$j\ 40$	$6{,}530 + j3800$
5-1-1	660.1	94.1	8.9	−.03	$1.12 - j\ .124$	$.882 + j.098$	$8{,}820 + j980$	− .2	$j\ 10$	$8{,}820 + j\ 990$
5-1-0	660.7	93.9	10.0	.08	$1.10 + j\ .332$	$.833 - j.252$	$8{,}330 - j2520$	.4	$-j\ 20$	$8{,}330 - j2540$
5-0-9	661.3	94.0	11.4	.22	$1.11 + j\ .91$	$.538 - j.442$	$5{,}380 - j4420$	1.0	$-j\ 40$	$5{,}380 - j4460$
5-0-8	662.1	94.2	12.45	.325	$1.13 + j1.35$	$.365 - j.435$	$3{,}650 - j4350$	1.8	$-j\ 80$	$3{,}650 - j4430$
5-0-7	662.9	94.6	13.65	.445	$1.17 + j1.85$	$.244 - j.386$	$2{,}440 - j3860$	2.6	$-j120$	$2{,}440 - j3980$
5-0-6	663.6	95.1	14.75	.555	$1.22 + j2.31$	$.179 - j.339$	$1{,}790 - j3390$	3.3	$-j150$	$1{,}790 - j3540$
5-0-5	664.3	95.8	15.95	.675	$1.29 + j2.81$	$.135 - j.294$	$1{,}350 - j2940$	4.0	$-j180$	$1{,}350 - j3120$

TABULATED COMPUTATIONS.—*Continued*

(1)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Oscillator Setting	log f_p	log Q_p	Q_p quadrants	z_1 mech. abohms	tan $\angle z_1$	$Q_p + 1$ quadrants	$\theta_1 + \theta_B$ hyps.
5-1-6	2.81743	0.90078	*7.9576*	1510 + *j* 3240	2.15	*8.9576*	.0302
5-1-5	797	132	*7.9675*	2030 + *j* 3650	1.80	*8.9675*	.0280
5-1-4	828	163	*7.9732*	2890 + *j* 4130	1.43	*8.9732*	.0260
5-1-3	869	204	*7.9807*	3830 + *j* 4410	1.150	*8.9807*	.0261
5-1-2	913	248	*7.9888*	6400 + *j* 3980	.622	*8.9888*	.0277
5-1-1	962	297	*7.9978*	8690 + *j* 1170	.135	*8.9978*	.0250
5-1-0	999	334	*8.0046*	8200 − *j* 2360	− .288	*9.0046*	.0248
5-0-9	2.82048	383	*8.0136*	5250 − *j* 4280	− .815	*9.0136*	.0257
5-0-8	093	431	*8.0225*	3520 − *j* 4250	−1.207	*9.0225*	.0290
5-0-7	142	477	*8.0310*	2310 − *j* 3800	−1.65	*9.0310*	.0292
5-0-6	187	522	*8.0393*	1660 − *j* 3360	−2.02	*9.0393*	.0304
5-0-5	233	568	*8.0479*	1220 − *j* 2940	−2.41	*9.0479*	.0308

fifth figure is shown in columns (14) and (17), as it is best to retain it in using the tanh chart to determine $\theta_1 + \theta_B$.

The constant vector, $z_d + z_1$, to be subtracted from the corrected values of z'', column (11), to determine z_2 is 130 $- j180$ mechanic abohms. This is obtained by plotting the corrected values of z'', drawing the circle giving the closest approximation to the points, k in Fig. 13, and passing a diameter, cd, through the center of this circle parallel to the resistance axis. The vector, oc, drawn from the origin to the nearer intersection of the diameter with the circle k is the vector $z_d + z_1$. Subtracting this vector from each point gives the values of z_2, column (15). The tangent of the slope of the vector z_2 is then computed for each point, column (16).

With these values and the values of $Q_p + \underline{1}$, it is possible to use the tanh chart to determine $\theta_1 + \theta_B$, according to the method discussed in connection with Fig. 7.

The average value of $\theta_1 + \theta_B$, column (18), is 0.0277 hyp. Hence the angle of the upper air tube is $0.0277 + j\underline{9}$. at resonance. The value of z_2 at resonance is 8830 mechanic abohms. The value of tanh $.0277 + j\underline{9}$ is $36.1 + j0$. Hence $a^2 z_o$ is 245 mechanic abohms by equation (22). These results differ slightly from those given in the Table of results, as those values are the mean values for both first run and check run.

List of Symbols

A, vector force factor of vibratory system (dynes per abampere $\angle$).

a^2 ratio of diminution of acoustic impedance (numeric).

B_m, motional susceptance of vibratory system (mhos).

b, linear susceptance of an acoustic conductor (mechanic abmhos per cm.).

c, a constant.

c', a vector constant.

f, impressed frequency (cycles per sec.).

f_o, impressed frequency giving upper tube resonance (cycles per sec.).

f_p, impressed frequency at an observed point (cycles per sec.).

G_m, motional conductance of vibratory system (mhos).
g, linear acoustic conductance of a uniform air tube (mechanic abmhos).
I_m, maximum cyclic value of alternating current (abamperes).
j, $\sqrt{-1}$.
L, length of air column (cm.).
L_d, damped inductance of vibratory system, millihenries.
L_f, free inductance of vibratory system, millihenries.
L_m, motional inductance of vibratory system, millihenries.
L_o, length of upper air column giving resonance (cm.).
L_p, length of upper air column at an observed point (cm.).
M, total mass of diaphragm within clamping circle (grams).
m_d, equivalent mass of diaphragm (grams).
m, an integer.
n, number of wave-lengths at which upper tube is resonating.
p_o, quiescent air pressure (baryes).
Q_o, imaginary part of hyperbolic angle of an acoustic tube conductor at a resonant point (quadrants).
Q_p, imaginary part of hyperbolic angle of an acoustic tube conductor at an observed point (quadrants).
R_d, damped resistance of vibratory system (ohms).
R_f, free resistance of vibratory system (ohms).
R_m, motional resistance of vibratory system (ohms).
r, linear resistance of an acoustic conductor (mechanic abohms per cm.).
r_d, mechanic resistance of diaphragm (mechanic abohms).
S, area of cross-section of air column (sq. cm.).
S_d, elastic coefficient of diaphragm (dynes per cm. of displacement).
T, temperature of air (° C.).
V, velocity of sound in free air (cm. per sec.).
v, group velocity of sound waves in an acoustic tube (cm. per sec.).
X_d, damped reactance of vibratory system (ohms).

X_f, free reactance of vibratory system (ohms).
X_m, motional reactance of vibratory system (ohms).
x, linear reactance of an acoustic conductor (mechanic abohms per cm.).
x_d, mechanic reactance of diaphragm (mechanic abohms).
x_{mo}, maximum cyclic amplitude of vibration of center of diaphragm (cms.).
Y_m, motional admittance of vibratory system (mhos).
Y_m', motional admittance of vibratory system (abmhos).
Z_d, damped impedance of vibratory system (ohms $\angle$).
Z_f, free impedance of vibratory system (ohms $\angle$).
Z_m, motional impedance of vibratory system (ohms $\angle$).
Z_o', diametral value of motional impedance (abohms).
z'', mechanic impedance of vibratory system (mechanic abohms $\angle$).
z_A, acoustic impedance of an air column at A assuming uniform amplitude of vibration (mechanic abohms $\angle$).
z_A', acoustic impedance offered by an air column to a vibrating diaphragm (mechanic abohms $\angle$).
z_d, mechanic impedance of diaphragm to motion (mechanic abohms $\angle$).
z_o, acoustic surge impedance of a uniform tube conductor (mechanic abohms $\angle$).
z_v, virtual mechanic impedance due to eddy currents set up in the diaphragm of an ordinary telephone receiver (mechanic abohms $\angle$).
z_1, lower acoustic impedance or that offered by air chamber below diaphragm (mechanic abohms $\angle$).
z_2, upper acoustic impedance or that offered by air chamber above the diaphragm (mechanic abohms $\angle$).
$\alpha = \alpha_1 + j\alpha_2$, linear hyperbolic angle of a uniform acoustic tube conductor (hyps. per cm.).
β, an angle (degrees).
β_1, β_2, small positive angles (degrees).
γ, ratio of specific heat at constant pressure to that at constant volume for air (numeric).
Δ, damping factor of a vibratory system (hyps. per sec.).

δ_A, δ_B, position angles (hyps. $\angle$).

$\theta = \theta_1 + j\theta_2$, hyperbolic angle of an acoustic conductor (hyps. $\angle$).

θ_B, real part of position angle of terminal load (hyps.).

λ, length of sound waves (cms.).

ρ, quiescent density of air (gms. per cc.).

σ, terminal load at end of an acoustic conductor (mechanic abohms $\angle$).

ω, angular velocity of impressed alternating current (radians per sec.).

ω_o, angular velocity at apparent resonance of diaphragm (radians per sec.).

ω_1, ω_2, quadrantal angular velocities (radians per second).

$\angle$, denotes vector quantity.

$\sim$, cycles per second.

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PROBLEMS IN THE COLONIZATION AND SETTLEMENT OF TROPICAL SOUTH AMERICA

By ADOLPHO LUTZ

(*Read April 28, 1927*)

For the present paper I chose a subject which needed no previous literary researches, impossible to accomplish in a short time, while I could fall back on my personal observations and an experience acquired during many years in different parts of Brazil, a country which owns the greatest share of tropical South America, and is fairly typical for the rest.

Only part of the country limited by the two tropics owns a really tropical climate, which implies an average annual temperature of not less, and generally more, than 21° centigrade, while the daily observations often mark more than 30° in the shade and may come within a few degrees below blood heat. This torrid zone may extend beyond the tropics, but between them it is limited to low vertical elevations, as the average temperature rapidly diminishes with increasing altitude, at a rate of about one degree for every thousand feet. Where there are high mountains after climbing a few thousand feet we may reach a delightful climate, but if we continue to ascend, we encounter quite unpleasantly cool and cold places. An aviator, starting in a tropical country for a high altitude record, would soon experience arctic cold, while the spectators were still stewing in tropical heat.

Even before the advent of Europeans, what there existed of higher culture in tropical America was localized in countries where cooler mountain climes were available for the capitals. Such conditions are outside the question we attempt to explain, to wit, that is why such large extents of tropical country should

continue to be undeveloped while the population of the earth is steadily increasing, so that several countries are already congested, and clamor for expansion complaining under the plea that their food production is not progressing, while their population is augmenting. Humanity is like a large family, crowded in an enormous house of which many rooms are not used.

This paradox, like so many others, is explained by historical and political conditions. Humanity is not a united family which might distribute the unoccupied rooms among its members, according to their common interests, but rather resembles a family in which most of the members think only of themselves and are always ready to quarrel with the others about their pretended rights, besides looking down with contempt on their poorer relations.

Tropical America has a very interesting pre-Columbian history, the investigation of which is making great progress, due in large part to North American scientists and corporations. However, after the advent of Columbus the whites did not trouble much about the rights of their primitive American relations, whom they despised as heathen. They claimed practically the whole of the newly discovered lands, although some of these had to be conquered first. By decree of the pope, South America was divided between the Spaniards and Portuguese by a boundary line which is still respected.

The problem to whom tropical America belongs is actually solved. Not even the most unknown parts are unclaimed, while quite a number had *two* claimants, as the limits indicated by hardly known rivers and by faulty maps were not well defined. Of late, most of the litigations have been finally decided, although some still remain in existence. Even some states of the same country have their "contestados."

According to the customs of their time, the Spaniards and Portuguese kept the new possessions strictly for themselves, looking upon them as a source of revenue for the king and an occasion of enriching his protégés. The principal object was to gather from the inhabitants as much gold, silver and precious stones as possible, and to discover new mines.

Those treasures were for the most part claimed by the crown, to whom they brought large revenues, although a great part of them were intercepted in times of war. Agriculture, though indispensable as a means of support for the soldiers and the other people in the service of the crowns, was not considered as the principal end. By using the information of the natives and their methods of traveling, the unknown countries were explored, in the hope of discovering mineral wealth. The "bandeirantes," who often had Indian mothers and spoke their language, lived in Indian fashion during their expeditions. These lasted for many months, sometimes even for years, and led to a general knowledge of the explored regions. The tropical forests, which yielded no mineral wealth, were little explored and only along some of the rivers.

The Indians were also employed to do the necessary work, but they were not greatly to be relied upon, even under compulsion. This led to the introduction of negro slaves, who showed more endurance and greater aptitude for hard work. They added a new element to the ethnological collection, which was further varied by frequent cross-breeding. The pure whites, and those who pretended to be such, despised manual labor, which was considered an attribute of slavery. The negroes certainly had a very large share in opening the country while slavery lasted, but after its abolition they showed little inclination for going on with the work.

Except in some restricted countries, the Indian element lost its importance with the advent of the negroes. In the more progressive parts, the Indians tended to vanish, partly owing to their slight resistance to diseases. In most places genuine Indians are never seen now, except as deputations from tribes in the interior, and then they form an object of curiosity; nevertheless, many members of the population still show some traces of Indian features. The genuine Indians, tame or wild, are found only in remote places, where, of late, tribes have been discovered who still lived in the stone age, while other unknown tribes may exist even now. The Indian languages are known only to scientists, or to people directly dealing with

the tribes. They are hard to learn and very varied. In Paraguay alone, where the population is in great part composed of Indians, a kind of "*guarany*," which the Jesuits introduced as a general language, is still commonly spoken. On the river Paraná the Jesuits gathered large numbers of Indians under their protection and organization. These settlements were quite prosperous, until the attacks of slave hunters caused the Indians to migrate down the river, where large numbers of them succumbed to malaria. The organizations dissolved when the Jesuits were expelled, although missionary work on a smaller scale is still done by other orders in some remote parts of tropical Brazil.

The mother countries treated their American colonies in a rather stepmotherly way, giving only scant consideration to the interests of the permanent inhabitants. Owing to the great distances and the dangerous journeys, no king of Spain or Portugal ever visited them until Dom João Sexto, through fear of Napoleon, transferred his court from Lisbon to Rio de Janeiro. After his return his place was taken by his son, who afterwards, under pressure, declared the independence of Brazil. The Spanish colonies also separated from the mother countries little more than a century ago, so that the whole of South America, with the exception of the Guianas, became independent. Some former attempts of the French and Dutch to settle Brazil ended with their final defeat by the Portuguese; but the Dutch began the scientific investigation of the country and some of their racial features are still found amongst the population of the region which they formerly occupied.

The three Guianas, although colonies of highly cultivated nations, do not seem to develop rapidly, because of climatic and other difficulties which have not yet been solved.

The Latin republics, to which Brazil has belonged since 1889, would not think of abandoning their rights to the land, and it is evidently from them that the development of tropical South America must come. However, none of them are suffering from overcrowding. On the contrary, most of them

have parts which are still undeveloped, although they have milder climates, and are often less distant, promising better remuneration with less danger to health.

Since the establishment of independence the dog-in-the-manger policy has to a large extent been abandoned. Free transoceanic navigation and international trade, the development of mineral resources, the opening of railway lines, the improvement in seaports, and the organization of many kinds of industries, have brought a large infusion of foreigners, some of whom have become naturalized and founded families. Foreign descent is shown throughout Latin America by many names, even of statesmen, although political rights are of course limited to citizens. However, naturalization is easily obtained and comes automatically to the children of foreigners born in the country.

Even before the complete extinction of slavery about fifty years ago, some South American states tried colonization with foreign elements, recruited from the poorer classes of European nations, with the double aim of furnishing working hands to the plantations and of forming an independent class of small farmers, which might supply the towns with their products. As in other places too much attention was paid to the number and too little to the quality of the immigrants, but the final result was, that the states which received the most colonists are to-day the most flourishing. However, all of these states enjoy favorable climatic conditions. The torrid zone received few immigrants, and in some of the warmer places colonization failed through unhygienic conditions. Just now, the Japanese are investigating the region of the Amazon with a view to colonization, and it will be interesting to learn what they think of it. In British Guiana and in Trinidad are many Hindoos who, at least in the latter place, seem to do well. They may easily find it an improvement on some of their own countries.

Large parts of the tropical coast line of Brazil offer relatively favorable climatic conditions, being cooled by almost constant breezes. As there are a number of good natural seaports within easy access, through which the imports and ex-

ports have to pass, quite a number of larger and smaller towns have developed. These were for the most part founded long ago and have a history. Bahia was the first capital, and Pernambuco was developed by the Dutch. As impenetrable forests were not prevalent, most of the zone being rather dry, communication with the interior was not so difficult to establish. In some parts, rivers formed an easy way of access. Now there are several railways of considerable length in connection with the landways and waterways of the interior, which extend to quite distant zones. The landways used for mule caravans, called "tropas," and for the transport of cattle, are generally pretty bad and become impassable after heavy rains, but we are now at the beginning of the development of automobile roads and their extension is one of the problems of the future, together with the development of the railway system and of river navigation.

It is not generally known that a considerable part of northern Brazil is rather dry and subject to periodical droughts, which at long intervals may assume the character of tremendous catastrophes, when the cattle die for want of water and food, and the starving inhabitants have to emigrate in large numbers, in order to save their lives. This happens principally in the state of Ceará. Although the emigrants might abandon these regions and settle in safer places, they generally end by returning.

Great quantities of food supplies and large sums of money have to be spent on such occasions, and the mortality, aggravated by epidemic diseases, is very large. To meet the expenses, the whole country has to contribute, since the individual states are not rich enough, and it is often difficult for help to come in time.

The federal government has spent great sums on the problem of preventing such catastrophes, by establishing barrages to retain the water in the rainy seasons, and by digging wells, and by opening communications. The question of dry farming has also been considered. However, there re-remains for all the dry regions the sanitary problem of keeping

the drinking water from pollution, as the limited water-supply in dry seasons is used promiscuously for all purposes and is open to every contamination. This has already led to the prevalence of *Schistosomum Mansoni* in those regions, and the prevention of this and other diseases due to contamination of the water-supply, must become an essential part in the sanitary program of the future.

The Rio de San Francisco, even in dry times, carries a considerable quantity of water, but close to the river there is an arid zone, which produces very little and almost seems a desert. As in Egypt, some cultivating is done, after the annual inundation, on the banks and islands which have been submerged; but, unlike the situation in Egypt, there is no general irrigation. I have traveled on the main river and its tributaries to a great extent. Some of the banks are less arid, but only the Rio Corrente, in which the strong current permitted the use of waterwheels, was used for irrigation. The results were extremely good and the formerly almost treeles land became covered with trees and vegetation, showing what could be accomplished in other places.

From Pirapora to a point beyond the junction with the Rio das Velhas the banks of the river are still normally covered with green vegetation, but then they become progressively more barren, although, strangely enough, some writers speak of this country as most fertile. On the lower part of the course which is mostly made in sternwheelers, wind is almost constant and could be used as power for irrigation. Here we see an extensive formation of sandbanks and dunes, while the water, which higher up is very muddy, becomes somewhat clearer.

Pirapora, where the navigation begins, is connected with Rio de Janeiro by a railway of 1,000 kilometers and in length while Joazeiro, where it ends, is connected with Bahia by another long line running through some very arid country. The tributaries of the São Francisco river drain a very extensive region, which transports its various products by means of them. One cannot help thinking that the production ought to be much larger, if proper irrigation was used. The popula-

tion in the numerous small towns along the river is mostly poor, with predominating black elements, showing little desire and capacity for progress. If a big corporation started irrigation and planting on a large scale together with canneries for fish and fruit, and established a few good roads, the country might become very flourishing. However, this would require much more capital than could be furnished by the native population.

Here is a region, situated well within the tropics, between the eighth and thirteenth parallels, which when developed, could support a much larger population than at present. With proper sanitary control, it might be made tolerably healthy. Below Joazeiro the river does not allow steam navigation, and finally, after a still long course, it forms the celebrated fall, or rather falls, of Paulo Affonso, where any amount of power could be obtained without spoiling the picturesque effect of this quite unparalleled cataract. This marvel of nature ought to be made more accessible to tourists who wish to appreciate its full beauty. Below the falls is a short railway, joining the river where it again becomes navigable for smaller craft, which alone are capable of passing the somewhat dangerous bar at the mouth of the river.

While the dry countries of the northern part of Brazil, although insufficiently developed, are nevertheless known and are accessible by the ordinary ways of traveling, conditions are very different in the vast tropical rain-forest, which surrounds the Amazon and its tributaries, apparently unbroken and with very little vertical elevation. Here the maps show only a dendritic system of affluents of all sizes. A certain number of small towns and settlements, bearing in part Portuguese, in part Indian, and in part saints' names, are grouped for the most part along the larger and more navigable rivers, but not close together. At a distance from the rivers, no names appear on the maps, since all the intervening space is practically unknown.

The problem of surveying those nearly unknown zones seemed hopelessly difficult only a short time ago, but the use

of aerial navigation, and of photography, makes the solution comparatively easy. The last Hamilton-Rice expedition was the first to use a hydroplane, with very satisfactory results; and of late, de Pinedo, went in *one* flight over the territory where a strategic telegraph line had been established a few years before at an enormous outlay of time, work and money. It should soon be possible, not only to correct the existing maps as to the courses of the rivers, but in particular to ascertain the distribution of forest and campos, swamps, plateaus and hills, in order to devise new ways of communication with some of the most remote places.

In the forest country mineral riches were not available, nor was there any agriculture. The vegetable products, among them some valuable drugs, were all obtained by "*industria extractiva*," which means the gathering of nature's spontaneous offerings, generally distributed over a large expanse, which is difficult of access. Among these, rubber proved to be the most important, so long as it fetched high prices. The rubber trees, belonging to the genus *Siphonia* and various species, not all equally valuable, grow along the riverbanks from Pará upwards to widely remote tributaries, mingled with other vegetation and never in large groups. The hope of finding new and good locations has led to a general knowledge of those rivers which were not too much obstructed by rapids, but not, as a rule, to permanent settlements. All the traveling was done by water, and only at high water could many of the places be reached, even by small craft. If they failed to return in time, owing to some unusual conditions, the crew was obliged to wait for the next year's high water. This happened so often that they went already prepared to make plantations, in order to get the necessary food supply.

Navigation by sail, oar, or punting, was necessarily slow, while the distances were often enormous. The foodstuffs, which formed the base of alimentation, were generally inadequate and the prices of the most common articles increased with the distance. Conditions generally were very much as they used to be in the goldfields. While traders and the

governments made a great deal of money from the high prices of rubber, the unfortunate rubber gatherers often spent more than they earned. As the latter could not leave without paying for the necessities of life, obtained on credit at very high prices, many of them remained practically in a state of slavery without any hope of improvement, besides being the victims of the prevalent diseases. When the monopoly of rubber was lost and the prices went down, there was a general state of depression, and many places were abandoned. The remedy might be found in siphonia plantations, like those of Ceylon, where they have given good results, but the expense and the risk are believed to be too great; also such plantations would have to be located near to the centers of population. Unless the prices improve much or some other profitable "*industria extractiva*" turns up, there seems to exist no incentive for further development, or even for the keeping up of the most remote, and more or less temporary, settlements.

On the Amazon itself and on some of the larger tributaries, there is of course steam navigation, mostly carried on by national companies. Foreign navigation was not permitted in former times, and even now only a few transoceanic steamers run up to Manáos. The places between Pará and Manáos were not at all up to date when I last saw them. The climate is very warm all the year round and there is little to make life comfortable. Manáos contains only a few streets which might be transplanted from a modern city.

The river Amazon is very wide, but comparatively shallow, and by no means so easy to navigate as might be expected. There is no perceptible current between the mouth and Manáos, except at Obidos, where the river is narrowed at low water to a width of 800 meters with proportional depth. The influence of the tide extends all the way up, a distance of over 860 miles. The water is always muddy, so that the bottom cannot be seen, even in shallow places. Islands of all sizes, some of them very long, are exceedingly common, so that both banks are rarely seen, and even then are very distant one from the other. There are considerable floods, so that all the

piers must have at least two stories, and the houses near the river are built on poles. The high water brings considerable changes by excavating the higher banks and carrying away parts of them, together with the trees which grow on them. The channels are perpetually changing, and the unbroken and monotonous vegetation which covers all the land makes orientation difficult for any one who does not thoroughly know the river. Even the steamer journey from Pará to Manáos still takes several days, and in traveling to more remote places by smaller craft, as Wallace, Bates and Spruce did in their well-described journeys, the amount of time spent is enormous. The total length of the Amazon alone is considered to be about 3,000 miles.

River navigation might be much improved through circumventing the rapids by railways, roads or channels, blowing up obstructions, and employing fast boats with flat bottoms and aerial propellers, instead of the present sternwheelers burning wood. In more prosperous times hydroplanes might come into general use.

There are practically no railways to shorten the enormous distances in Amazonia. A short line is running from Pará to the Atlantic coast through a rather favorable country. Another was begun near Alcobaça and there is a third one going round the falls of the Madeira, to give the Mamoré navigation from Bolivia an outlet on the Amazon. Both of them had to contend with difficulties similar to those met by the Panama railway.

The river is still the only means of communication. Horses are kept only on cattle ranches, as on the isle of Marajó. These had to be constantly renewed, as they succumbed to the *Mal de cadeiras*, a trypanosomiasis of the *capivaras*, transmitted from them by horseflies. The *capivara* (*Hydrochoerus capibara*) is by far the largest rodent, attaining the size of a hog and quite amphibious. It is very prolific, and becomes quite numerous when not persecuted. The horses used to come from the island of Caviana, where the disease was not prevalent. With the extermination of the

capivaras, and treatment by modern methods, the disease might be kept down; but the horses cannot be used in the forest region for want of food, and would be useless without roads.

For the moment there is only one railway projected to connect Rio de Janeiro with the Amazon by way of Pirapora. The Rio de San Francisco is already bridged over, and the preliminary exploration has long since been done. The work is not expected to progress very quickly and will not affect the opening of the unknown forest region, although it may furnish some valuable experience.

The opportunity of telegraphing by cable or communicating by railway is already quite fair, and the wireless telegraph seems especially invented for communicating over unexplored countries. The benefit of these institutions might be increased by lowering the charges.

With a large number of people employed in the *industria extractiva*, in trade, and in navigation, the Amazon region has never produced enough foodstuff to make importation unnecessary. Travelers in more remote places often find it difficult to obtain anything, except the poorest and most monotonous diet, so that *beriberi* was extremely prevalent. It is characteristic that chickens had to be imported and were sold at very high prices, even in Manáos, although they generally represent one of the principal resources of most settlers in tropical countries. Vegetable products, so easy to grow in the tropics, never formed an article of exportation, except in the case of cacao, which was not used at the places of its production. Brazil nuts form an article of local consumption and of exportation, but also belong to the *industria extractiva*. The castanheiro is one of the tallest wild forest trees. I have not heard of attempts to make plantations of this tree which might give results only after a very long time. While mentioning the *industria extractiva*, one might as well ask, why the woods themselves should not be more exploited. There is no doubt that if half the forest zone could be converted into roads, paths and clearings, the other half would benefit exceedingly, while enormous quantities of timber, fire-

wood, charcoal and paperpulp might be obtained. At present, the precious woods for cabinet work, which may be admired in exhibitions and in the palace of the president of Pará, are obtainable only in limited quantities; for in the Amazonian forests, with the exception of some palms and a few species of no value, the same kind of tree is never found in large groups nor near to the waterline, where, however, firewood may always be had. Even to obtain a good dugout, it is sometimes necessary to cut a long path from the river through jungle and forest, causing more work than the canoe itself. Any clearing, when not kept up, will in a few years be overgrown by jungle, while a path cut through the woods will take even less time to become impassable. The only way to keep up a clearing for agricultural purposes is still to cut down all the vegetation with the axe, let it dry as much as possible, and then set fire to it. On the ground covered with ashes and charred wood the first cultivating is done with the hoe, while the stumps and fallen trees are slowly rotting away. As any new plantation generally requires this preliminary work, and as agricultural machinery cannot be used, the small amount of tilled ground is easily explained. Better methods of exploiting the forests and clearing the ground constitute another problem of the future, the solution of which may, however, be expected with confidence.

As for animal products, there are a great many cattle raised on Marajó, on the low lands along the banks of the Amazon and in the region of the Rio Branco, which are shipped alive to Manáos and Pará. The pastures are not good, being subjected to drought and inundations, which cause the animals to perish in great numbers. The financial returns, however, are good, because the expenses are very small. Crossbreeding with the large *Mysore* zebus has given good results. Cattle raising is limited to the campos and llanos, also known as savannas or prairies, which are found for the most part to the north of the forest zone.

The campos or llanos, which have a dryer climate, are easier to explore and healthier than the forest zone. Still,

they do not seem to progress rapidly. The principal industry is the raising of cattle and horses, which demands much space when the pastures are not of superior quality. In some of these places we find the same problems as on the Rio de São Francisco.

In this connection I might mention still a fourth zone, which in some parts is quite developed; we may call it the swamp region. Actually, this is for the most part avoided. If the malaria problem becomes solved, these places might be used for the extensive cultivation of rice, which in some places already exists in a wild state. With it might be combined the raising of ducks, following Chinese methods.

Besides being the only way of locomotion, the Amazon and its tributaries furnish also most of the animal food of local origin. The game, even with good native hunters, is not so much to be relied upon and besides is for the most part found only near the water. The best supply of fresh meat is provided by the big turtles, which on certain occasions are collected on land in numbers and can be kept alive for any length of time. Their meat is wholesome, and even very good; and so too are the eggs, but it would be preferable not to collect these, as the plundering of their nesting-places leads to the rapid diminution of the turtles. There is also the pirarucú, a big fish of the size of the tunny, which, like the turtles, is caught when in the water by shooting it with arrows, so constructed as to form a sort of harpoon. The meat is rather coarse, but is dried in great quantities and often used as a staple food, although it meets with little approval from the consumers. There are better fishes, which are often caught in great quantities, but do not keep so well as the pirarucú. To prevent the extermination of the turtles, and to preserve some of the fishes in a more perfect way, may be considered another problem of the future.

After this rapid and incomplete sketch of the conditions prevalent in tropical South America, we may inquire into some of the principal obstacles to progress. The first of these is always the climate, which we are unable to change, though

we may mitigate its effects. While in our case, life is rarely threatened, heatstroke and similar conditions being rarer than in hot summers of many regions of the moderately warm zones, there is no doubt that the continual heat has an unfavorable effect on white men by producing excessive transpiration, which leads to prickly heat, continual thirst, loss of sleep and appetite, followed by neurasthenia. These appear for the most part at times and places with prevalent moist heat and warm nights, not relieved by cooling winds. White people can hardly be expected to reach their full bodily and mental capacity in places where mere existence is difficult. Though I do not doubt that they may maintain themselves unmixed through various generations, circumstances of a sanitary, social and economical nature have generally prevented the experiment, and isolated cases do not permit the drawing of conclusions. I have seen two white polar bears, kept in the zoölogical garden of Rio de Janeiro for years, and while admiring their faculty of adaptation, I should not consider them fit for tropical life.

For indoor work conditions may be so much improved as to make it comparatively easy, but much depends on local resources. Cooling the houses in warm countries is just as important as is heating them in cold climates. The problem of the dwellings is to obtain the maximum of ventilation with the minimum of insolation. Artificial cooling does not show much progress at present, but its improvement is one of the problems of the future. The most appropriate manner of clothing must be studied and it ought not to be influenced by fashion. Where hills or mountains are within easy reach, the families may live there and the working members may return after the day's work. The seat of the government, of large enterprizes and of health resorts may be located in similar places. Where those are not available, leave of absence might be given periodically, in order to give relief from the continual heat.

Good bathing arrangements, ventilators and a liberal supply of ice may mitigate the effects of the climate to a large extent.

Whoever is finally expected to do the work must be given a certain amount of comfort and satisfaction in his possessions. Appropriate, nice-looking cottages or houses must be substituted for the mud buildings which, unless whitewashed inside and outside, always look disreputable and harbor vermin, sometimes of a dangerous nature. Barefoot walking is risky in many ways. The water supply must be good and must be kept uncontaminated; appropriate privies, and washing and bathing arrangements, are of the highest necessity to keep up sanitary conditions. Ambition for a higher standard of life must be cultivated, principally among the younger generations for which end schools and cinematic demonstrations may be used. Although the improvements above mentioned are just as desirable in cooler climates, their necessity is there more generally known, while their neglect is less fatal. It is only right that some of the progress should come from the places where it is most urgent, just as the studies on tropical diseases benefit other countries, where the same diseases exist without prevailing to a similar extent.

The problem of obtaining a good water supply, or at least pure water for drinking, must be solved according to circumstances, by using springs, simple or artesian wells, and as a last resort by filtering, individually or collectively. Under critical conditions, it is better to discard the use of unboiled water for tea and coffee and to use imported, still or aerated, drinks. This advice is meant for the traveler, because the inhabitants generally are not exacting, but drink with great enjoyment the water on which they navigate, or, worse still, get it from the shore, where the animals enter for drinking and bathing and where all the washing is done. The worse the conditions, the less they seem to mind them. It is true, as has frequently been pointed out, that the evil effects of this are not necessarily evident, but an importation of cholera or typhoid fever soon shows the fallacy of that argument, as I have had occasion to witness.

Of all the problems in the development of tropical America none is more important than the prevention of certain highly

fatal diseases, not so much those which are peculiar to, or restricted to, the tropics, as those which are especially prevalent in hot countries. Although not a necessary consequence of high temperatures, they are nevertheless dependent upon them. So long as the real cause, and the way of spreading, of these diseases were not known, but were subject to erroneous theories and speculations, all the sanitary methods adopted proved insufficient. However, the last fifty years have gradually led to such phenomenal advances in our knowledge, that we may soon be able to consider the tropical diseases as the most accessible to prevention and cure.

Now-a-days the parasites that produce tropical diseases are for the most part known and can be seen under the microscope when they circulate in the blood, thus confirming the diagnosis. Many of these organisms, adapted to the heat of the human body, require a temperature not much inferior to that of the blood, when they leave it, to undergo further evolution. This takes place in some bloodsucking insect or tick, the temperature of which corresponds to that of the air. When this is not warm enough, development is interrupted or altogether stopped. The tropics are more affected in this respect, because bloodsucking insects are active there all the year round, while the temperature does not drop below the critical point. In moderately warm climates the favorable temperatures last only part of the year and the epidemics show spontaneous interruptions.

In warm countries people may harbor in their tissues a small and very thin roundworm capable of remaining in the lymphatic or blood vessels. It is now known as *Filaria Bancrofti*. The larvae are found in the circulating blood but, curiously enough, only at night time. Considering the possibilities of their leaving the body without drying up, Manson, while in China, discovered this periodicity, which reminded him of the time of activity observed in the most common house-mosquito. He soon verified further states of evolution in the mosquito, when it had absorbed human blood with larvae, and Bancroft, in Australia, completed the life-cycle by showing

that the fullgrown larvae reached the mouth-parts of the mosquito, and invaded the human body, on the occasion of the next bite.

This observation, wonderful in itself, became the starting point for a succession of other brilliant discoveries in human and animal pathology, made by an army of investigators of many countries. To-day there are many handbooks and periodicals dedicated to tropical medicine, which is taught as a postgraduate discipline in several schools of different countries. In connection with it, protozoölogy developed as a branch of importance, helminthology and entomology added new chapters to their investigations, while parasitology and medical zoölogy became collective disciplines to an extent previously unexpected.

The notion that parasites of the human blood might depend for their evolution and transmission on mosquitoes and other bloodsucking insects or ticks, in the first instance added to our knowledge of malaria, caused by protozoas, which inject the red blood-corpuscles. Their animal nature was first recognized by Laveran in Algiers. Subsequently, several Italian scientists extended his observations and led to the establishment of three species, found also in America and distinguished by their morphology and by the symptoms which they produce. The most deadly form of malaria is especially prevalent in the tropics. Ross established that the transmission of malaria parasites, found in men and in an allied species of birds, was accomplished by mosquitoes serving as intermediate hosts, while Grassi showed that in human malaria the transmitters belong to the genus *Anopheles*. Of that genus many species are known all over the world, being generally divided into many genera, which form the subfamily *Anophelinae*. All of these are more or less under suspicion, but no other mosquitoes capable of carrying the infection have been discovered. Most of the *Anophelinae* breed in stagnant water, in swampy or periodically inundated regions, but the malaria of mountain forests is transmitted by a species breeding in the water collected in epiphytic plants, as I was first able to show in Brazil.

There are many ways of attacking the problem of tropical malaria: by the treatment and isolation of the patients; by protecting healthy persons from the bites of infected mosquitoes; by destroying the breeding-places of the larvae, by drainage or by filling up the swamps, by the removal of aquatic vegetation, and by stocking with small fishes; by covering the surface of the water with a film of petrol; by treating the water with aniline dyes or other substance which poison the larvae; not to mention some more theoretical proposals. According to circumstances, they all may prove useful, but all of them together are not sufficient to guarantee the sanitation of a place under really difficult circumstances. Quinine does not cure the chronic cases which cause the infections; screens are expensive, perishable and inappropriate for the places where the poor people sleep; mosquito nets make the temperature unbearable on really hot nights, and the larvicide measures can be used only on a small scale. Intelligent people living in well screened houses may escape infection, even where malaria is prevalent, but for the poorer classes inhabiting periodically inundated countries, a successful antimalarial campaign depends on the finding of a remedy which will cure the chronic infections, as well as the acute ones. The speedy solution of this problem of experimental therapeutics might come any day, considering the present activity in trying new chemical compositions.

More circumscribed foci of malaria in well administrated places may be made to disappear by drainage, by filling up pools or treating them with larvicides, as shown in Lagos and at the canal of Panama, both of which places had a very bad record and are situated well within the tropics. Clearing away of trees and jungle has driven out malaria from some places that I know. Rigorous exclusion of all infected workmen by medical examination ought to be the first step in all large undertakings, such as railway, road and bridge building, even in uninhabited countries, which through want of these and other precautions regularly lead to epidemic malaria. Prophylactic administration of quinine, and screening or dis-

infecting the sleeping quarters, where possible, may contribute to save the situation. The contractors must be made responsible for the sanitary measures.

Yellow fever is the most deadly infection transmitted by the mosquitoes. It was always noted that this disease was not directly contagious and did not spread in places where the temperature was relatively low, due to vertical elevation. In other places the epidemics ceased in the colder season and reappeared with the heat. Foreigners were especially subject to it, while many natives seemed to acquire immunity, independent of a manifest attack. All the hunting for the responsible germ, in which I myself spent much time, had given no practical result, as happened in other diseases attributed to organisms too small to be perceived, even with the microscope. Disinfections, avoiding the drinking of water from the local supply, and the prophylactic use of arsenical preparations proved of no avail. The possibility of a certain kind of mosquito being connected with the diasease was pointed out at least twice, the last time by *Finlay* in Cuba; but his experiments, made in an infected region were not sufficiently convincing. However, his intuition was afterwards proved to be right by a commission investigating the disease in Cuba, though mostly composed of North Americans. By this time the idea of mosquito transmission had become more familiar and I, for one, accepted it at once, as I had gathered very strong circumstantial evidence in the first epidemic of Campinas, in the State of São Paulo. At my suggestion mosquito prophylaxis was established in the epidemic centers of this state (where I was director of the Bacteriological Institute) and an illustrated description of the yellow fever mosquito was published. However, a repetition of the experiments in Cuba was considered desirable. Of five volunteers bitten by mosquitoes infected from the same patient three had yellow fever symptoms; the director of the sanitary department and myself showed an immunity acquired without manifest attack. Other ways of contamination gave a negative result, as in Cuba. Finally a French Commission from the Pasteur

Institute studied the question more extensively in Rio de Janeiro and their results were considered final.

The first of the long standing endemic centers to become free from yellow fever by mosquito prophylaxis was Havana, due to the activity of Gorgas, and the next was Rio de Janeiro, where Oswaldo Cruz successfully overcame all the obstacles. Since then, yellow fever has rapidly diminished, and its last hiding places are being watched by the Rockefeller Commission and by the local authorities. In my opinion, it will still take a long time of assiduous watching to get rid of every remnant, but, at any rate, the disease has ceased to be an obstacle to the settlement of the tropical countries, at least on our hemisphere, although what might happen, if it should be carried to the other side of the globe, where dense populations and mosquitoes abound, is quite another question.

Dengue is another epidemic disease, transmitted by the same kind of mosquito, and beginning with the same symptoms. As its mortality is practically nil, its transmission has been extensively tested on human beings, positive results being constant where there was no previous attack. We must familiarize ourselves with the idea that, at least in the tropics, infectious diseases are just as likely to be transmitted by bloodsucking insects, as by contact or by infection of the water.

As I have explained on another occasion, leprosy in its spreading behaves just like yellow fever, the difference being of course that the latter is an acute disease, while leprosy is an eminently chronic one. The circumstantial evidence of the mosquito transmission of leprosy is overwhelming, but the experimental proof cannot be given until a safe cure, or a susceptible animal is found. One might expect that the mere suspicion ought to be sufficient to make the admittedly desirable mosquito prophylaxis obligatory in the treatment and sanitary regulation of leprosy; but so slowly does the human mind adapt itself to new ideas, that this is not even seriously thought of by most of the people who consider themselves leprologists.

The long list of diseases transmitted by bloodsucking insects in the tropics, or outside of them, is steadily increasing and comprises also some of the most deadly diseases of domestic animals. To enter into a description of most of them would lead too far, but this much we learned in the last decades and principally during the war, that what people used to call vermin are still more objectionable from the sanitary than from the aesthetic point of view, and that mosquitoes, sandflies, blackflies, gadflies, ticks and fleas are not only nuisances, but really dangerous. For this reason the study of all these temporary parasites has become quite an extensive science, to which I have dedicated a considerable part of the last thirty years. Insects and ticks, sucking the lymph or blood of man and domestic animals, are very numerous and may be rated at several hundred species in the warmer part of South America. What gives these studies a special interest and value, is their relation to diseases, not only of man, but also of the most important domestic animals. The investigation of animal diseases has contributed enormously to the understanding of human pathology, while the morphological, and principally biological studies of the temporary parasites has become one of the most interesting chapters of entomology.

There is, however, a large number of tropical diseases independent of insects, but due to the contamination of soil and water by human excrements. Some of these are connected with protozoa living in the intestines. The most important is the *Endamoeba histolytica*, producing a kind of dysentery and sometimes abscesses of the liver, now fortunately quite amenable to medical treatment. Others are due to worms and of these the so-called *Ankylostomiasis*, or hookworm disease, is the most prevalent. Although curable by several methods, its ultimate extinction will be obtained only by sanitary reforms which means breaking whole populations from inveterate habits. As is generally known, the Rockefeller Commission has been working in this line for years in many countries, in combination with the local authorities. As the disease is mostly acquired by walking barefoot on infected soil,

the use of boots or shoes is a real necessity. These will also afford a protection against sandfleas, against infections following accidental wounds such as tropical ulcer, and also against snake bites.

The list of problems is not exhausted, but I trust that a general idea of the most important ones has been given. I have not stated new facts, but have tried to recall and emphasize the special difficulties connected with the development of new regions. This will come of itself when it is really needed, but the process is necessarily slow. Thus we gain time for educating new generations and introducing many of the improvements which we may confidently expect from the general and rapid progress of humanity.

I have limited my discussion to the conditions in tropical South America, because I knew, though not being an ocular witness, of the splendid work the United Fruit Company has already done in Central America. It has given a practical demonstration of what may be achieved in developing new countries, when the medical and sanitary part of the task is duly considered.

PLATONISM AND THE HISTORY OF SCIENCE

By PAUL SHOREY

(*Read April 28, 1927*)

Humanists, when admitted to the fellowship of scientific men, claim that they too are investigators and are trying to discover unknown truth by the exact methods of science.

This is not the occasion for controversy. And I have no wish to disparage that aspect of the so-called Geisteswissenschaften. But I do not think that it is the chief service of humanism. The term science cannot be used here in the sense in which it applies to physics and chemistry, for the simple reason that there is less possibility of verification and less progress. You cannot test in the laboratory the opinions of a scholar about the return of the Heracleidæ or the Platonic philosophy. They may be estimated by fashion, by his general reputation, his skill in self-advertisement, or at best, by the verdict of a dozen or score of possibly jealous experts dispersed through the world. And though we talk of progress in classical studies, and there are of course new discoveries in archæology, the value of a classical text, essay or book, depends much more on the personal scholarship, good sense and literary taste of the writer than on the fact that he writes in the year 1925 and not 1850.

But the chief values of humanism are cultural rather than scientific. And so far as they are scientific, I would stress rather the training of the judgment than the investigation and discovery of fresh truth. Critical judgment of the meaning of books, documents, the written word, is one of the latest, rarest, and most easily lost of human attainments. Lawyers cultivate it with great precision in a narrow field; experts, including men of science, must have something of it in their

own specialty. But the majority of educated men, including lawyers and men of science, do not even know that they lack it in the broader domain of literature and general criticism of life. They do not know that they cannot trust themselves to understand what they read (especially in the literature of the past) or to translate, quote, interpret, or apply it correctly and rationally.

Classical studies will not of themselves impart this discipline to recalcitrant minds. But rightly taught, there is no better educational instrument for training this kind of judgment than the classics, and no field in which it is more needed than in the interpretation of the two great literatures separated from us by the chasm of the Middle Ages and composed in languages we call dead. Misunderstandings of the classics naturally multiply in an age which has so many more pressing things to think about. Trivial errors, which amuse the scholar and delight the gloating pedant, are of no concern. But in the light of the new interest in the evolution of human thought and the history of science, the perpetuation and broadcasting of error about Greek philosophy is a more serious matter. I am not now speaking of doubtful points of metaphysics or the philosophy of history but of the quite definite and demonstrable misapprehensions—"howlers" in fact—which the index of almost any book of recent philosophy, science, or the history of science that mentions Plato, Aristotle and the pre-Socratics at all, will reveal. Writers who expect us to accept on faith their would-be scientific and critical interpretation of special and general relativity, symbolic and mathematical logic, the quantum theory, the constitution of the cell, the structure of the atom, the evidence for the inheritance of acquired qualities, will gravely refer us to a page of Plato or Aristotle for an idea that isn't there, and that intelligent and attentive reading of any good translation even, would have shown them is not there. The limits of my time and the courtesies of the occasion forbid me to mention names. But I have them, and there are astonishingly few exceptions to what may be thought a petulant generalization. The fact

surely indicates some defect in our education, or else the failure of the cautious scientific temper to function in this field. But this is not a paper on education, and in place of further generalization I will confine myself to one widespread and frequently reiterated error about Plato.

Though there are honorable exceptions, it is currently taught that Platonism is the antithesis of the scientific spirit and that Plato is a reactionary in relation to the evolutionary and mechanistic philosophies of the pre-Socratics, and a dreamer, spinning the world out of his inner consciousness, as contrasted with the fact-loving Aristotle.

The association of Platonism with superstition is an historical fact and perhaps a natural tendency. Maeterlinck begins with vague, poetic, Neo-platonic idealism and ends by faith in the performances of the kluger Hans horse. Driesch begins with postulating something he calls entelechy, supplementing mechanism in organic life, and ends with the acceptance of telepathy, telekinesis, and even clairvoyance. William James, who, however, was not a Platonist, begins with assuming an indetectable something that throws its sword into the scales of the will, and ends with faith in the revelations of the medium, Mrs. Piper. And the history of Platonism through the centuries would supply abundant further illustration. The later Neo-platonists practiced levitation. The Cambridge Platonist and poet More, opponent of the materialist and mechanist, Hobbes, is one of the most insistent defenders of the superstition of witchcraft and demoniac possession. The witty Lucian, a great admirer of Plato the literary artist, in his extremely modern dialogue, "The Liar," represents a Platonic philosopher as swallowing all the ghost stories which the Epicurean rejects. No wonder, says Lucian smartly; a man whose eyes are sharp enough to discern the Platonic Ideas can of course see spooks.

The indictment, then, is partly true of historical Platonism. But it does not fairly fit Plato. As an ethical religious teacher he uses the religious language and symbolism of his time precisely as that of their age is used by Schleiermacher, Renan,

Matthew Arnold, Emerson, and the American men of science who protest that their brand of evolution is creative, emergent, or, in the latest phrase, holistic, and not mechanistic. There is no time to prove that here. But it could be proved, and there is no passage in Plato's writings which, correctly understood in its context, is incompatible with this interpretation. Every one of the ten or a dozen passages misused by superstitious Platonism and by too many scholars to-day, is in its context plainly the merely literary, decorative satirical or allegorical employment of imagery and illustration borrowed from the Mysteries, from Orphism, Pythagoreanism, or popular religion.[1] It is not Plato who is unscientific, but the readers who are too uncritical or too impatient to apprehend his clear intentions.

Plato's fluid, literary, and edifying use of religious language has been taken literally, and developed into rigid theological and mystical systems by superstitious Platonists throughout the ages. And the distaste which hard-headed mechanistic positivists and behaviorists feel for such writers has been transferred to Plato. It is allowable only to those who are willing to condemn as superstition anything less than dogmatic mechanistic materialism, and to include in the condemnation not only Plato and the Platonists but all the liberal theologians and men of science to whom I have referred. Plato himself never opposes specific scientific enquiry,[2] or substitutes the final for the mechanistic cause. He merely, like our creative evolutionists, expresses his broad faith that the final cause, if we knew all, might well be superposed on, and found compatible with, the mechanistic secondary cause.

[1] This is what Diès, *Autour de Platon*, calls somewhat cryptically "la transposition platonicienne."

[2] There is one apparent exception, exploited by Goethe and Schopenhauer in the controversy on the theory of color. In Timaeus 68D after stating the apparent results of the mixture or blending of colors, he adds: "But if anyone should try to test all this by experience (in fact) he would have misconceived the difference between human nature and the divine. God alone both knows how and is able to combine the many into one and break up the one into the many. But no man is now capable of doing either of these things, or ever will be." This language bears a certain resemblance to modern denials of the possibility of the chemical synthesis of organic products. But however that may be, it has nothing to do with our present point, the alleged misuse of the final cause by Plato.

The only truth in the commonplace contrast between the facts of Aristotle and the dreams of Plato is, first, that Plato was a mathematician and Aristotle a biologist, and a biologist naturally collects more little facts than a mathematician. And, secondly, that Plato was an artist and Aristotle an encyclopædist—and an artist digests his facts while an encyclopædist catalogues them. The eighth book of the "Republic," so greatly admired by Macaulay, if compared with the catalogues and classifications of Aristotle's "Politics," will illustrate this contrast. One of its opening sentences reads almost like an ironical forecast of Aristotle's work and Herbert Spencer's sociological tables. (544*c*) "Can you name any other form of government—I mean any that presents a clear distinctive type? For principalities and purchased sovereignties and other such constitutions intermediate between those named can be found in even greater numbers among the barbarians than among the Greeks. Many indeed and outlandish are the kinds that are reported."

None the less, Plato's writings as a whole are quite as concrete, as rich in experience, as free from *a priori* logic chopping and unverifiable deduction as Aristotle's. Both inevitably fell into many errors; but Plato as a whole is far nearer the point of view of recent science than Aristotle. The contrary opinion is due to a few sentences excerpted from Aristotle's biological works, the philosophic generalizations of which are in fact mostly derived from Plato.

After clearing away these misconceptions it remains to review some specific passages of Plato that have been thought to be anti-scientific in temper, and lastly to indicate his specific service to, and place in, the history of Greek science.

The chief and first exhibit for the prosecution is the so-called "Timæus," which stands quite apart from the logical, ethical, and social questions that are the main substance of the Platonic dialogues. It is a philosophic prose poem. It is what one ancient critic called it, a hymn of the universe. It belongs, broadly speaking, to the type of literature represented by the fragments of the pre-Socratics, Lucretius' poem

"On the Nature of Things," Cicero's "Dream of Scipio," Poe's "Eureka"—which is however on a much lower plane though by no means as absurd as it is usually represented to be—and Bergson's "Creative Evolution." It is an attempt to set forth in pregnant, generalized and impressive language the picture and conception of the universe that results from the science of a given age and the personal philosophy, the religion and the cosmic emotion of the writer. Historically, the "Timæus" is by the range of its influence in antiquity, on the Christian Fathers, in the Middle Ages, the Renaissance, and even as late as Goethe and Emerson, who admired it greatly, the most important of all books of this class. With that we are not concerned.

Obviously such a book cannot be critically or fairly estimated by the simple method of extracting some of its errors about scientific fact and cataloguing and displaying them apart from their context, and the purport and tone of the whole. It is intelligible only when critically interpreted in its relation to the pre-Socratics and the science of Plato's time—not to speak of the Platonic philosophy. Yet it is by disconnected extracts taken usually not from the original, but from Grote's arid summary, that the orators of anti-Platonism and too many men of science pass a hasty judgment not only on the "Timæus" but on Plato and Platonism.

They do not even pause to ask which of the errors were common to the age and believed by Aristotle, which are only jest and irony, which are fancies that the writer himself says lack verification, which are allegories and symbols, either of moral and religious principles, or of scientific ideas that, despite their mistaken illustrations, are valid to-day even as some of Plato's philosophy of language is sound, despite the false etymologies that accompany it.

I of course cannot present a critical interpretation of the "Timæus" here. I can only indicate the lines of a juster appreciation. It is, as I have said, Plato's "De Rerum Natura," his attempt to rewrite from his own point of view the pre-Socratic cosmogonies, his unconscious anticipation of Lucretius' great poem.

The often uncritical scholars, philosopher and poets of the Renaissance were right in feeling this essential identity of poetical tone and sentiment about nature and the universe in spite of the deep underlying philosophic dissidence. More literal-minded critics think first of the difference, which is that the pre-Socratics were all tending toward that materialism and elimination of design which were so dogmatically and eloquently expounded, by Lucretius,[1] while Plato's purpose is to write his cosmogony in terms of design as far as the scientific knowledge of his day would allow. In so doing, he works partly as a literary artist, and partly as a philosophic student of contemporary science. As an artist he introduces the suggestion, the symbolism, the implication of design in ways which do not affect his treatment of specific scientific principles and particular scientific facts, but which are as irritating to dogmatic mechanists as they are pleasing to convinced teleologists. That is something that can be understood and allowed for only by critics who are able and willing to distinguish a writer's explicit affirmations from the suggestion by tricks of style of his personal preferences.

This insistence on teleology deepens the suspicion aroused by the mystics who take Plato's name in vain, and heightens the unfavorable contrast with Epicurus and Lucretius. The more thoughtful men of science would, I think, admit that their objection is not so much to the teleological idea in itself, as to the absurdities of its detailed application, or the misuse of it to discountenance experimental endeavours to ascertain the mechanism. From both of these faults Plato (though not always historical Platonism), is free. Except in obvious jest he does not use the Bridgewater treatise and Xenophontic type of argument from design; he does not oppose a mechanistic explanation of particular phenomena or substitute for it a vague teleological affirmation. His teleology, such as it is, is superadded to mechanism, and does not displace it.

[1] Cf. my article on Greek Philosophy in Hasting's "Encyclopædia of Religion and Ethics." Professor Dewey's generalization that Greek philosophy was from the beginning the hand-maid or apologist of theology is apparently a transfer to the pre-Socratics of the modern positivist's feeling about historical Platonism. Heiberg (geschichte der Mathematik und Naturwissenschaften, p. 1) states the matter correctly.

The censure of the mechanistic philosophy of Anaxagoras, in the Phædo [97c ff.], has been misunderstood. It is often bitterly denounced as fanatical, anti-scientific obscurantism.[1] The full meaning of the "Phædo" and its many secondary intentions are not to be apprehended by a hasty reader. Plato says in effect, that he would prefer a teleological evolution but that he cannot discover it himself, and that he was disappointed in not finding it in a philosopher who proclaimed that Nous or Intelligence was the first principle. Aristotle, whom the denouncers of Plato contrast favorably with this passage of the "Phædo," says much the same. Plato goes on in the "Phædo" to explain a non-committal logic of causation which is in fact the origin of the Aristotelian syllogism.[2] The first half of the "Timæus" is a poetical, symbolic exposition of the teleological philosophy of nature which the "Phædo" despaired of as strict science.

With these allowances the concrete science of the "Timæus" is quite on a level with the best thought of Plato's time. The unavoidable scientific errors are less vital than those of the Aristotelian Astronomy and physics, and are half redeemed by Plato's insistence throughout that he is only telling a probable tale that admits of no proof.

It is only the main conception, and not the insignificant and irrelevant details, that concern us here. The world has been drawing up outlines and making superficial paraphrases and summaries of the "Timæus" for the past two thousand years. More than a dozen have been published by eminent scholars or men of science or philosophers in the last twenty years, not to speak of mere popularisers. I need not add to their number. Any one of them will serve for a general introductory notion. Few, if any, can be trusted to be critical throughout.[3] Passing over the poetry, the symbolism and the

[1] Lange, "History of Materialism," 1, 9; H. G. Wells', "History (1920)," 1, 358.

[2] Cf. my paper on "The Origin of the Syllogism," *Classical Philology*, Jan., 1924.

[3] That valuable work, "Le système du monde," of Pierre Duhem (not to name living scholars) draws far-reaching conclusions from simple mistranslations.

I., p. 59, douées d'une puissance antagoniste qui les tire vers lui; p. 37, l'être universel, understanding therby the universe as a whole. The meaning is simply: everything that is—anything that really is, and on p. 82 the misapprehension of *enarges*

artistic framework, I turn at once to one of the central scientific conceptions. The peculiar form of the atomic theory associating the molecules, so to speak, of the four traditional elements with four of the five regular solids, and atoms with certain types of triangles, is like the teleology of the first half a deliberate, arbitrary and consciously artistic choice. But it is nevertheless, as Professor Whitehead said, nearer the point of view of the atomic theories of to-day than anything in either Aristotle or Democritus. The same may be said of Plato's discussion of space, time and change.

These analogies have been approached in recent years from two directions: in German dissertations on Plato and Democritus or Plato and Mathematics [1] and in modern books, on the philosophy of the sciences, of which Professor Emile Meyerson's "De l'explication dans les sciences," (Paris, 1921), and his "La déduction relativiste" (Paris, 1925) are the most convenient types.[2] It is pointed out, for example, that Kekule, Vant Hoff and Werner have tried to explain otherwise inexplicable chemical isomeries by the properties of the hexagon, the tetrahedron and the octahedron.[3] I cannot here undertake to distinguish the fanciful from the solid in those German dissertations,[4] or to examine all the analogies between the "Timæus" and the "Republic" and the most recent physics in M. Meyerson's thoughtful book. But I may be permitted to point out that the general notion that Plato's theory of matter is a more philosophical type of atomism is to be found already in old Cudworth and that my own paper on "The Interpretation of the Timæus" in *A. J. P.*, 1888, Vol. IX, anticipates most significant points in these recent discusions and protests in advance against some of their errors. I will illustrate this by an extract [p. 416–17] in which some of the anticipations are italicized for brevity, and which will at

[1] Engeborg Hammer Jensen, Demokrit and Plato, *Archiv für geschichte der Philosophie*, Bd. XVI. Eva Sachs, "De Theaeteto . . . . mathematico," Berlin, 1914. Cf. *infra*, p. 77, n. 3.

[2] Cf., also P. W. Bridgman, "The Logic of Modern Physics" (Macmillan, 1927), who, however, says nothing of Plato or history.

[3] Meyerson, "De l'explication, etc.," p. 298–300.

[4] The hypothesis that Plato discovered Democritus when he had written as far as 47E in the "Timæus" and then suddenly changed his plan is of course an absurdity.

the same time serve as a résumé of some of the most significant scientific ideas of the "Timaeus."[1] "In the explanation of material things we accept the four elements from contemporary science, though they are obviously not elementary in any proper sense. The *only real elements involved in objects of sense are space and the mathematical relations. So far we accept the results of Democritus.* But the atomists cannot really claim to have proved their specific doctrines with regard to the shapes and sizes of their atoms. *The atomic chemistry has nothing to go upon but the obvious analogies between a smooth body and a soft sensation, or between a rough, jagged body and a harsh sensation.*[2] In order, then, to maintain against the theory of flux and vortex, our principle that God geometrizes[3] and introduces proportion and harmony wherever possible, we shall arbitrarily base our atoms on an *a priori* geometrical construction (53DE). But we shall willingly yield the palm to the surer science that shall demonstrate a better method (54A). Furthermore, the atoms of Democritus are particles of unqualified matter in space, and suffice in themselves for the production of all qualities. *We recognize no abstract matter apart from space. Our atoms are purely mathematical relations. They explain only the connections and changes of things. The essential qualities that make each thing what it is are derived from the absolute eternal idea. We were forced to assume such fixed eternal unities in logic, and we cannot dispense with them here* (51B). In neither case are we able to state clearly now their virtue is infused into transient things. Assuming these atoms and the cosmic agency of the Demiurgus, a few general forces will enable us to give a plausible analogical explanation of the chief phenomena brought before us.

Among these are the attraction of similar bodies (63E, 53A), the constant revolution of the heavens (58A), which maintains a plenum (58A, 79B, 80C), sets up a περίωσις (80C,

[1] Cf. Bridgman, *op. cit.*, p. 43–4, 93, etc.

[2] Cf. Meyerson, "De l'explication," Vol. I, p. 281 ff., especially: la chaleur du feu est de même, expliquée par les angles aigus de ses particules. Mais les atomistes font appel" a la même ressource.

[3] I of course was and am aware that Plato himself never said this in terms.

79C), and makes impossible a positive *actio in distans* (80C, ὁλκὴ μὲν οὐκ ἔστιν οὐδενί ποτε);[1] the far-reaching distinction between mobile and stable bodies (64AB), and the principle of the stability of the homogeneous and the instability of the heterogeneous (57A, 58C). In human physiology and anatomy the prime fact is the distinction between the intellectual, emotional, and appetitive or vegetable soul, and our study should be directed to tracing the designs of our makers in providing instruments for the first, discipline for the second, and the necessary conditions for the harmonious working and due subordination of the third. Diseases are explicable on purely physical grounds; they are of the nature of living organisms, and are to be treated as far as possible by flexible regimen. Moral defect is in the main due to removable physical conditions (87B). There is a certain continuity throughout the animal kingdom indicated by rudimentary organs (76DE). The Democriteans evolve the higher from the lower by the operation of chance. Proof there is none, and we will therefore substitute for the guess of transmorphism the assertion of a metaschematism intentionally devised for ethical ends by the moral ruler of the world."

There is space for only a few further illustrations of recent discussions of the analogies which I brought out in the page reprinted above. Professor Meyerson, *e.g.*, shows that Plato, like Descartes and our newest physics tends to reduce matter to configuration in space.[2] He admits that this is the drift of recent physics, but thinks that there and in Plato this tendency ignores the irreducible element of irrationality, that is of impenetrability by the deductive reason, in our experience. And so he approves Aristotle's criticism of what he calls the pan-mathematism of Plato. That, however, is to forget what I had already shown, that Plato does not actually construct reality out of configured space. Space is the recipient, Plato says, in some marvellous and unexplained way, of forces, powers, potentialities, that enter into it from the world of Platonic Ideas, and the mathematical construction is solidified

[1] Cf. Bridgman, *op. cit.*, p. 46.
[2] Cf., *e.g.*, "De l'explication," I., p. 179.

before it becomes an elemental atom or molecule.[1] Now as Professor Meyerson himself quotes with approval leaders of science to the effect that our scientific laws like Plato's Ideas are more truly real things than are the so-called real objects of sense,[2] he could not consistently object to the Platonic solution of the mystery or recognition of the irrationality which I pointed out but which he seems to overlook.

Professor Robin, on the other hand, seems to forget that the physicist of to-day stands helpless before the same problem and the same antinomies.[3] And so he cannot refrain from constructing out of the dubious testimony of Aristotle and his own interpretation of the "Philebus" and "Timæus" a final metaphysics for Plato which will solve them. He apparently would repeat of Plato what Meyerson, p. 161, naïvely says of Einstein: "l'appareil entier de la déduction einsteinienne a besoin de l'interpretation pour produire du réel." This recalls Zeller's lament that he could find in Plato "keine Ableitung des Sinnlichen," to which as far back as my doctor's dissertation I replied by asking him to name some satisfactory modern deduction of the world of sense from abstract metaphysical or mathematical principles. And similarly when writers to-day complain that Plato's atomic theory does not make it perfectly clear how he extracts quality from quantity I ask: what modern physicist does make it clear? Professor Robin writes ("La physique dans la philosophie de Platon," p. 50, italics mine): "Ainsi, quand le Timée explique le feu sensible à la fois par la configuration géométrique du feu élémentaire et par l'Idée du feu, il se place tour à tour au point de vue de la *réduction de la qualité à la quantité et à celui du rapport des qualités particulières à leur essence absolue.* Mais, en nous rappelant qui'l existe une science divine des principes d'où dérivent les relations mathématiques auxquelles se réduit la qualité sensible, *Platon, interprété à l'aide des témoignages d'Aristote,* semble nous indiquer en même temps que ces

[1] Tim., 56B: *stereon gegonos eidos.*

[2] Cf. Bridgman, p. 35.

[3] Cf., *e.g.*, Lucien Poincaré, apud Meyerson, "De l'explication," p. 23, and Meyerson, "La déduction relativiste," p. 12 ff., and Bridgman, *op. cit., passim.*

principes doivent aussi servir à expliquer la qualité intelligible, c'est-à-dire l'Idée."

It is more truly philosophical, as well as more critical, I think, to take Plato as we find him, with less expense of ingenuity in the interpretation, to read the Timæus as a philosophical poem shot through with suggestions and intuitions of science, and not to demand of Plato a metaphysical completeness, a symmetry, an ultimate dogmatic consistency, which are to be found not in the serious scientific thought but only in the system-mongering of to-day.[1]

The other chief stumbling-block to the modern scientific reader is the discussion of mathematical astronomy and physics in the programme of the higher education in the "Republic" (VII., 523 ff.). The fact that Plato bases the higher education of his guardians on a severe course in science is of itself a sufficient refutation of the notion that he is an enemy of science. But into that we need not enter. Plato had not and could not have our utilitarian reasons for making science the staple of the higher education. It would be possible to answer those who condemn Plato for this by quotations from many leaders of modern science who teach that disinterested intellectual curiosity is the highest motive of science, and that, even when utility results in the end, it is fatal to the scientific spirit to keep this end slavishly in view. They point to the error of Comte, who said that astronomy outside our solar system could never be of any use, and that we ought not to speculate about the physical constitution of the stars. Science too is a kind of idealism and must hitch its wagon, the chariot of progress, to a star. However that may be, there was and could be in Plato's age no clear premonition of the Baconian mastery over nature which is the miracle of our day.[2] Plato values science as mental discipline, and more specifically as developing the power of pure abstract thought.

[1] On the earlier attempt of Mr. Henry Jackson to find parallel metaphysical systems in the "Philebus" and "Timæus," cf. my paper on "Recent Platonism in England," *A. J. P.*, 1888, Vol. IX., p. 274. Its arguments are applicable to all such endeavors, and until they are answered it is unnecessary to amplify them.

[2] Yet cf. Plato's anticipation of the chief Baconian formula in my paper on "The Origin of the Syllogism," *Classical Philology*, January, 1924, p. 17.

Mathematics, to begin with, compels the child to think not of ten apples or a score of beans, as our schools of education would have him do, but of the numbers themselves.[1]

When Socrates in the "Republic" turns next to astronomy, the other speaker, taking Platonic metaphors literally, as sentimental Platonists still do, says, "Astronomy certainly turns the eye of the mind upward." But Socrates ridicules that star-gazing conception of astronomy and says that the upward gaze of the soul means the study and contemplation of abstract ideal mathematical relations and principles in their application to solids in motion. It is easy for a hasty modern reader to mistake that for a rejection of observation and fact and a proposal to deduce the phenomena of astronomy *a priori*. But Plato is not thinking of that. He is in some sort predicting the mathematical astronomy of to-day. That is of course not the whole of our modern astronomy. But it exists and is a fulfillment of Plato's prophecy.

Plato expresses this idea by saying in anticipatory correction of the Aristotelian error which dominated the Middle Ages, that the actual movements of the heavenly bodies, however wonderful, fall in precision far short of the true movements of real swiftness and real slowness in true number and in all true geometrical figures moving in relation to one another and bearing along in their movements their content. The meaning of this is sound science. But the language of Platonic idealism seems to us upside down. The movements and the mathematical relations are spoken of as the real things and the sun, moon, planets and stars are things put into these movements and carried along by them. And as if that were not enough, he adds: We must study astronomy as we do geometry, in generalized problems abstracted from particular matter, and let the things in the heavens alone. The meaning is quite harmless. It is an unconscious prophecy of modern mathematical astronomy. But the exaggerated Emersonian emphasis of the phrase "let the things in the heavens alone" [2]

[1] Cf. my paper on "Ideas and Numbers," *Class. Phil.*, April, 1927.

[2] For a modern utterance nearly as drastic, cf. Bouasse, apud Meyerson, "De l'explication," I., 125–6. "Il semble que je rabaisse étrangement le rôle de l'expérience, le principe découvert, elle n'interviendrait plus que pour vérifier les déductions de la géométrie. Dans l'espèce son rôle était par conséquent inutile, etc."

will continue to convince matter-of-fact readers careless of the context that Plato is the prototype of those Aristotelian professors of Italy and France who refused to look through Galileo's telescope at the phases of Venus and pronounced the spots of the sun specks in the glass, because they are not mentioned in Aristotle. Duhem, pp. 96 ff., interprets more reasonably and is essentially right in saying that Plato would substitute the *true* astronomy for the astronomy of observation.

It may be thought that these analogies are merely the uncritical enthusiasm of a teacher of Greek for his favorite author. And if you turn to the best scientific work on ancient astronomy, T. L. Heath's "Aristarchus of Samos," that suspicion will be confirmed: "Plato (he writes, p. 139) was a master of Method and it is an attractive hypothesis to picture him as having at all events foreshadowed the methods of modern astronomy." But in rejection of this hypothesis he quotes Plato himself as saying that "the person who thought that the heavenly bodies should always move precisely in the same way and show no aberration whatever would properly be thought absurd, and that it would be absurd to exhaust one's self in efforts to make out the truth about them." Besides, Heath adds, observation is excluded by the words "we shall let the heavens alone."

The first of these statements is true, and as we have seen is a remarkable anticipatory protest against the superstitious belief of Aristotle and the Middle Ages in an unchangeable heaven.

The statement that it would be absurd to exhaust one's self in efforts to make out the truth *about* them is in tone, at least, unfair to Plato's meaning. What Plato really intended to say, is, I think, that the true philosopher or scientific astronomer will in every way try to ascertain the truth or reality *of* these things, meaning loosely the more exact mathematical principles to which the apparent movements are only an approximation.[1] The whole means only that he will study

[1] Cf., *e.g.*, "Phædo," 74, and "Republic," 510, cf. Bridgman, *op. cit.*, pp. 17, 34, 61 and *passim*.

mathematically those abstract or ideal motions to which the same word "true" is applied in the preceding two or three paragraphs. He is not saying colloquially that it is foolish to try to make out the truth, that is the apparent facts, *about* the sensible movements of the heavenly bodies. I personally believe that an irregularity of construction, of which there are other examples in the Republic, determines this meaning.[1] But the usual construction, that given in Adam's edition, for example, will suffice for my purpose in Duhem's fairer interpretation (p. 95), "Il regardera comme un insensé . . . celui qui s'efforce de toutes manières de *saisir la vérité en* ces choses accessibles aux sens," the tone of which is quite different from that of Heath's "make out the truth *about* them." With no intention of disparaging Mr. Heath's admirable and indispensable book, I will use another paragraph of it to illustrate the caution that these studies require, and at the same time to develop a little further the topic of Plato's knowledge of astronomy. Speaking of alleged contradictions in Plato, he says on p. 171, "the description [in the Timæus, 39] of the 'wanderings' of the planets as 'incalculable' in multitude and marvellously intricate, is an admission in sharp contrast to the assumption of the spirals described on spheres of which the independent orbits are great circles,[2] and still more so to the assertion in the "Laws" [821–2] that it is wrong and even impious to speak of the planets as wandering at all since each of them traverses the same path, not many paths, but always one circular path." "For the moment," he adds, "Plato condescends to use the language of apparent astronomy, the astronomy of observation; and this may remind us that Plato's astronomy even in its latest form is consciously and intentionally ideal in accordance with his conception of the true astronomy, which lets the heavens alone." Even when this is literally correct, its tone is again, I think, misleading. "Incalculable," which is Archer-Hind's translation, conveys

[1] Briefly, I think the subject of the infinitive *zetein* seek (530 B.) is the true astronomer, and that it does not depend on *atopon* absurd, so in 492 C. *φήσειν*, if that is the text, refers to the youth. Cf. also the use of *kalein* in 581 E.

[2] The "spirals" of the "Timæus," 39 A, are clearly explained by Duhem, p. 55–7.

to us an idea of the negation of science not in the original. *Amechanon* hardly means incalculable except in the sense incredible or wonderful. The alleged contradiction with the "Laws" is of no significance for Plato's development. For Heath himself admits that the supposed contradiction is almost as sharp with the immediately preceding paragraph of the "Timæus" itself. The phrase "Plato condescends to use" is an unwarranted sneer, and the remark that the astronomy of the "Timæus" and Laws is ideal in the sense of the "Republic," which lets the heavens alone, is an equivocation which hardly rises above the dignity of a pun; the astronomy of the "Republic" passage is ideal in the sense that it is to be abstract and mathematical. If the word ideal is applied to the popular sketches given in the "Timæus" and "Laws," it can only mean that so much astronomy as Plato deems essential for general culture and philosophy is there stated in simplified form with omission of technical details and problems that are as yet not solved or that the readers he has in mind could not understand. There is no real contradiction between the statement in the "Laws" that the planets are not really errant or wandering bodies, and the remark in the "Timæus" that the full explanation of their wanderings (their apparent wanderings, that is) would be too complicated and time-consuming for the present work. Duhem, p. 97, 101–102, explains this well, but is misleading when he brings in the doubtfully genuine Epinomis to justify a third stage in Platonic astronomy, the theological. Plato undoubtedly in his theodicy [1] holds that the heavens declare the glory of God and that an undevout astronomer is mad. He wishes his rulers to know enough astronomy to feel and teach this, and everybody to learn enough not to blaspheme—which is his way of saying, not to misrepresent grossly the movements of the planets. But to call this a "theological astronomy" is to give the impression of superstition, from which Plato is free.

To return to the "Republic." The application of similar principles to a postulated mathematical study of acoustics [531] is too complicated for this brief summary. After

[1] "Laws," Book X.

studying as much as I could understand of Henry J. Watt's "Psychology of Sound," I am inclined to think that Plato has here overshot himself, and has been led into error by the assumed analogy with celestial mechanics, which is in a certain sense and to a certain degree an *a priori* science. But there is, I suppose, no such *a priori* Kantian intuition of intrinsically harmonious musical vibrations. It is a matter of observation and experiment. But I shall be glad to hear from some expert on this point.

In thus defending Plato against the charge that, because he emphatically demanded mathematics, he would have rejected observation and experiment, I might invoke the dangerous alliance of modern mathematical mysticism, which is only too ready to exalt Plato and Pythagoras before him for anticipating the proposed reduction of all philosophy and all science to mathematics.[1] I need not quote illustrations of a view which you meet everywhere in philosophical literature to-day. When it is not bluff, mysticism, or the mathematician's magnifying of his office, it can only be a prophecy of the final triumph of materialism. Philosophy and science can be swallowed up in mathematics only in so far as all qualities can be definitely correlated with measurable quantities. We may leave that consummation to the unknown future. There is no prediction of it in Plato; and the passages in which he praises mathematics and opposes measurement to guessing do not justify attributing it to him.

As for Pythagoras, of whom we know nothing, the wise reader turns the page when he sees that name, as Tyndall (or was it Huxley?) did when he saw the word 'polarity.'

But in these passages Plato has some notable anticipations of recent dicta of leaders of modern science on this subject, which are worth quoting. In "Republic" 602D, he says that measuring, counting and weighing are most gracious aids against the illusions of sense and the subjectivities of opinion; in "Philebus" 55E he says still more notably that if one

[1] Cf. Bridgman, p. 62, "There is no longer any basis for the idealization of mathematics"; and p. 63, "mathematics reminds one of the loquacious—orator who was said to be able to set his mouth going and go off and leave it."

divorces arithmetic, measurement and weighing, from the arts and sciences, that which remains is of little value—there is left only conjecture and guess-work and the exercise of the senses by empiricism and habit.

These utterances surely differ little from Lord Kelvin's statements that he understood a thing only when he could construct a working model of it, and that if you can measure a thing and express it by number, you have some knowledge of it, otherwise not; from Kant's declaration that the only part of any theory of nature that is scientific in the strict sense of the word is the quantity of mathematics which it contains; from Clerk Maxwell's statement that progress is symbolized in the clock, the balance and the foot-rule.

The histories of science, and especially of mathematics and astronomy, will give a fair general—though by no means always critical—notion of Plato's actual attainments. It would serve no purpose, and I have not space, to summarize the often doubtful details again here.[1]

Plato not only advocated education in science, but practiced it. Various steps in the progress of mathematics and astronomy are attributed to Plato as head of the Academy and director of research. He himself uses language that implies this conception of his function.[2] One legend assigns to him the solution of the Delian Problem of the duplication of the cube. His friend Theætetus, whose name is attached to one of the profoundest dialogues, apparently discovered and constructed some or all of the five regular solids.[3] The astronomer Eudoxus, whose theory of the celestial spheres prepares that of Aristotle and the Middle Ages, was a friend of Plato and attended his lectures. There is and can be, perhaps, no complete and critical account of the theory of the spheres, but Duhem [1.102 ff.], Heath, J. L. E. Dreyer,

[1] Cf., Heiberg, *op. cit.*, pp. 7–12 and 51–52 (slight). Heiberg, p. 51, accepts the notion that "Laws," VII, 821b–822c, is a late conversion to belief in the revolution of the earth on its axis. Plato's text does not justify the interpretation. Duhem, p. 88, rejects it. England's notes *ad. loc. discuss* the evidence and the probabilities.

[2] "Republic," VII., 528 B C, "Euthydemus," 290 C.

[3] Cf. "Die fünf platonischen Körper," Eva Sachs, in the Kiessling and Wilamowitz "Philologische Untersuchungen," Berlin, 1917.

"History of the Planetary Systems from Thales to Kepler," and the notes of Ross on Aristotle's "Metaphysics," Lambda 8, after the chief authority, Schiaparelli ("I precursori di Copernice nell' Antichità") will suffice for all practical purposes. The too common attribution of real physical spheres, crystal or otherwise, to Plato himself, is unwarranted, either by "Phædrus," 246–7, or by "Laws," Bk. V., to which, *e.g.*, Duhem (Vol. I., p. 39), refers. It belongs to the uncritical science of Chalcidius' commentary on the "Timæus." The Greek historian of astronomy, Eudemus, reported a tradition that Plato proposed to his students the problem: what hypothesis of regular and homogeneous movements will save the phenomena of the apparently irregular movements of the planets? The history of the phrase "save the phenomena," Milton's ignorantly contemptuous "to save appearances" has been sketched by an English [1] and written by a French scholar.[2] It proves conclusively that the more intelligent ancients [3] anticipated that most modern distinction between the working hypotheses of any science and ultimate material facts, a distinction which those, who tell us that Einstein has proved that space is bent and curved, forget [Cf. Bridgman, p. 176]. It would be superfluous to repeat that there is a school of philosophy which in reaction against nineteenth-century positivism is again trying to abolish that distinction, and insist that true physical relations must be relations of something. Many puzzled physicists feel in this way; they do not know how to answer their students' question: "If atoms are only charges of positive and negative electricity, what is it that is charged?" [cf. Bridgeman, p. 41 and *passim*] Meyerson quotes Sir Oliver Lodge as saying plaintively, quite in the logic of Plato's "Parmenides," [4] "Waves must be waves of something." Meyerson himself says ("De l'explication," I., 7): "La science est essentiellement ontologique—elle ne peut se passer d'une réalité posée en dehors du moi."

[1] J. B. Mayor, *Journal of Philology*, 1876, VI., 12, p. 171.
[2] Pierre Duhem.
[3] Duhem would even attribute the idea to Pythagoras himself.
[4] 132 B is each of their thoughts (*i.e.*, ideas) a thought of nothing?

As Herbert Spencer, refuting Berkeley, pathetically cries, "Should the idealist be right, the doctrine of Evolution is a dream."[1]

Further mathematical and astronomical detail would contribute nothing to the purpose of this paper, and can be readily found in the books to which I have referred.[2] They must be read with caution, with the precautions which this paper is intended to suggest.

The actual scientific knowledge of no ancient thinker, except perhaps some of the methods of the later mathematicians and some clinical observations of physicians, is of any concern to any working investigator to-day. The interest of the history of ancient science is either that of any evolutionary study of origins, or it depends on the significance that we attach to the anticipation by ancient thinkers of those large, general conceptions, which it is commonly said modern science first introduced into the world. That may be partly true, if we take for our starting point and standard of comparison the darkest period of the Middle Ages, as a too often quoted purple patch of rhetoric about *un homme d'autrefois* on the first page of Anatole France's "Jardin d'Épicure" does, and as Professor Dewey and the *Scientific American* did when they told us that nobody knew that the earth was round. It may be true in a sense, if we are thinking of the democratic masses of mankind whom modern means of communication make accessible to the popularization and propaganda of science for the first time. It is not true, if the comparison is with Graeco-Roman philosophy and the cultured few who have always had access to it. There are few, perhaps no, general conceptions

[1] But cf. Bridgman, p. 58: "I do not believe that the additional implication of physical reality has justified itself by bringing to light a single positive result.

[2] Cf. *supra*, pp. 167 and 177. I. L. Heiberg, "Geschichte der Mathematik und Naturwissenschaften im Altertum," in the Iwan Müller Handbuch, München 1925, is a résumé of 118 pages close-packed with facts and bibliography, which supersedes his similar contribution to the Gercke and Norden Handbuch, 1912, and makes further bibliography superfluous here. I may mention, however, E. Hoppe, "Mathematik und Astronomie im classischen Alterthum," Heidelberg, 1911, and the brief but clear outline of L. Laurand, "Manuel des Etudes grecques et latines," Paris, 1923. The Encyclopædia Britannica articles on mathematics and astronomy are good; the article on Plato is misleading.

of modern science applicable to religion, ethics, philosophy, education, or psychology, that were not familiar, often in more precise or more eloquent formulation, to readers of the pre-Socratics, Plato, Aristotle, the Stoics, Epicurus, Cicero, Plutarch, and so on down to the worthy Bishop Nemesius's treatise "On the Nature of Man" about 400 A.D. That is the thesis of a long-delayed book which I hope to complete in two or three years. Meanwhile, I shall be grateful for any friendly letter of challenge to match some significant large idea which the writer regards as the peculiar appanage of modern science. Plato alone rightly understood will usually suffice. But where Plato fails, some one of his successors will almost always guide us to any by-path of our latest speculations.

Once more, from the point of view of working science and material progress, and flying the Atlantic in a night, all this is of no significance. All depends on whether we are as much interested in the ascertainable history of the civilized human mind, as we seem to be in that of the hypothetic psychology of the cave-man. If we are, it is worth while to get it right.

Educated ancients, men like Cicero and Plutarch, understood the facts which I have related and knew that the real scientific men of antiquity were Platonists and Aristotelians, not Epicureans. The mere dogmatic assertion of atomic materialism, and the denunciation of every form of the idea of design could not in their eyes make a true scientific thinker out of a poet who supposed the moon and sun to be about their apparent size and who denied the possibility of the antipodes. Many also of the great scientific men of the Renaissance and the following centuries understood the matter and spoke respectfully of Plato's scientific attainments and conceptions. It is enough to mention Galileo and Leibniz.

The chief source of the opposite tradition is the Italian predecessors of Bacon and Bacon himself, who, in spite of his own immense debt to Plato, chose to prefer Democritus.[1]

[1] Heiberg, p. 12, accepts a similar view of Plato and Democritus from the German dissertations mentioned above, p. 167.

We need not here trace the recent fashion of exalting Lucretius and depreciating Plato as a reactionary further back than Macaulay's essay on Bacon and Lange's "History of Materialism." I have read Macaulay's essay on Bacon some dozen times because it is the source of more plausible errors in contemporary opinion than any other writing known to me except Spencer on Education and Wells' "History." But like most readers I never took time to test its quotations. Glancing at it again for this paper I re-read the page of declamatory denunciation of Plato for scorning all practical applications of mathematics; I looked up the references to Plutarch and found that Macaulay had missed the main point: Plato's chief objection was not to the practical use of mathematics in mechanics, about which Plutarch indulges in some idealistic rhetoric, but to the attempt to solve the problem of the duplication of the cube or of the finding of two means between given extremes [1] by the construction of a mechanical model instead of by pure mathematical reasoning. It is thus that the history of philosophy and science is written.[2] From Bacon, Macaulay, and Lange, this conception of Platonism as anti-scientific passed into the popular writings of Huxley and Tyndall, and so became a commonplace. On the other hand, Huxley says that Lucretius had drunk more deeply of the scientific spirit than any other poet, and Tyndall echoes him. Lucretius' materialism and his hostility to design account in part for this judgment. But the chief reason is the praise of science and the scientific man in Lucretius' poem, and the magnificent rhetoric of cosmic emotion outsoaring even Bacon, the modern orator of science. There could be no more striking proof of the ineluctable dominion of rhetoric even over the scientific mind.

Past the wall unsurmounted that bars out our vision with iron and fire
He has sent forth his soul for the stars to comply with and suns to conspire.

[1] Cf. Duhem, pp. 29–30.
[2] Cf., however, Heiberg, *op. cit.*, p. 8.

There are few sentiments which the man of science to-day is moved to utter in his exalted moods that he cannot find better expressed in Lucretius than in any modern poet, orator, or philosopher. Perhaps that ought not to be so, but it is. The modern poet of science may know more and versify an unimpeachable history of science:

> How many problems geometric
> Are easy now, thanks to the metric
> Trick of Descartes, who found a way
> To think them out by algebra.

That—I am translating, not inventing—I presume, is sound science. But when the man of science wishes to be thrilled and stirred, he goes back to his Lucretius and repeats of Newton, Darwin and Einstein what Lucretius says of Epicurus: "Nam si ut ipsa petit maiestas cognita rerum dicendum est, deus ille fuit, deus, inclyte Memmi." Hence, even the widely-read and cultured Doctor Osler, addressing the British Classical Association on "The Old Humanities and the New Science," while fairly appreciative of Plato's moral eloquence, overstresses the scientific importance of Lucretius, and complains that classical education in neglecting the "De Rerum Natura" disregards the scientific side of ancient thought. The classicists are not really negligent of Lucretius the poet. But having studied rhetoric more than their scientific colleagues, they are more nearly immune to it, less likely to be swept from their moorings by a blast of eloquence. And they know that Lucretius, like Bacon, is not the representative, but the orator, of science. The founder of ancient science was Plato, and it is from him that the world has received both the idea of science and the conception of a scientific education and the public encouragement of research.[1]

[1] "Republic," 528C.

COSMOPOLITANISM IN GERMAN ROMANTIC THOUGHT

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That the classic age of German literature at the end of the eighteenth century and the beginning of the nineteenth was altogether under the spell of cosmopolitanism, is one of the commonplaces of history. Lessing called patriotism a heroic weakness. The central idea of Herder's whole life was the conception of mankind as an organic whole, manifesting itself in a great variety of national types but held together by the universal progress toward perfection. Goethe relegated national hatred to the lowest stages of human development and claimed for himself a supranational state of mind to which the happy or sad experiences of another people appealed as strongly as those of his own. Schiller saw the mission of Germany in garnering the spiritual harvest of all ages and nations and transforming it into something still finer and higher. Kant found the hope of Europe in a League of Nations or a Super-State whose legal authority would ultimately overcome the rule of arbitrary and selfish might.

In contrast with this undoubted cosmopolitanism of the foremost representatives of German Classicism, German Romanticism has often been characterized as a breeding place of nineteenth century nationalism. And it certainly is true that some of the most characteristic productions of Romantic literature following the battle of Jena, such as Ernst Moritz Arndt's "Geist der Zeit" or Heinrich von Kleist's "Hermannsschlacht" or the Lyrics of the Wars of Liberation, betray an intensity of national passion unknown to the Weimar classi-

cists. Equally true, however, is the fact that even in this period of life-and-death struggle for the very existence of Germany some of the foremost leaders of Romantic thought, notably Fichte, Wilhelm von Humboldt and Schleiermacher, inspired as they were by the highest patriotic motives, and devotedly working as they were for a national rebirth, at the same time remained entirely loyal to the international ideals of their classicist predecessors, and indeed made an exalted cosmopolitanism the very cornerstone of the new nationalism. It is the combination of national and cosmopolitan elements in the thought of these three men, the regenerators of higher education in Prussia after the downfall of 1806, which I shall briefly discuss.

The transition from universally human to more specifically German ideals which, broadly speaking, was brought about by the catastrophe of 1806, has often been illustrated by the contrast between Fichte's "Grundzüge des gegenwärtigen Zeitalters" (Outlines of the Present Age) of 1804–05 and his "Reden an die deutsche Nation" (Addresses to the German Nation) of 1807–08. And truly, the contemptuous superiority with which the author of the "Grundzüge" speaks[1] of the "earth-born people who find their fatherland in the soil, in rivers and mountains and remain confined therein," whereas "the sun-fed spirit turns to and makes his home wherever there is light and right," seems separated by a wide gulf from the impassioned fervor with which the author of the "Reden" adjures his countrymen to remain faithful to the sacred trust of the ancestral home inherited from the past. But upon closer scrutiny we find that these two works of Fichte are after all not irreconcileably opposed to each other.[2] Even the seemingly extreme and absolute glorification of Teutonic character that pervades the "Reden" is not a radical break with the enthusiasm for the universally human that pervades the "Grundzüge." Even here Fichte does not lose sight of his maxim that "the best patriot always is also the best citizen of the world."

[1] *S. W.* VII, 212.

[2] Cf. Fr. Meinecke, *Weltbürgertum und Nationalstaat*, p. 92 ff. Bruno Bauch, *Fichte und unsere Zeit*, 2d ed., p. 30 f.

The goal hovering before Fichte's imagination in the "Grundzüge" is the bringing about of a state of mind which is to lead us from the present corrupted and diseased age to a new and higher stage of human development. All progress in the past was made possible only through the privations, sufferings, self-sacrifices of great individuals who lived and died not for themselves but for the common weal. This spirit of individual self-surrender to the needs of the whole should now be made universal. It should teach our thinkers and poets that they are only mouthpieces of a greater spirit than themselves and that they must not degrade its noble gifts into instruments of petty ambition or vulgar entertainment. It should keep our statesmen from the viciousness of arbitrary despotism and personal intrigue. It should place our political and social institutions upon the basis of mutual obligation of all toward all. It should safeguard the laboring masses against the whims of the ruling classes. It should transform the accumulations of the rich from being only the means of private self-indulgence into active forces of public betterment and progress. It should, in a word, help to create a commonwealth where all classes and all individuals devote themselves in free service to something larger than either individual or class—to humanity. "Nothing," these are Fichte's own forever memorable words,[1] "nothing can live by itself and for itself, everything lives in the whole, and the whole continually sacrifices itself to itself in order to live anew. This is the law of life. Whatever has come to the consciousness of existence must fall a victim to the progress of all existence. Only there is a difference whether you are dragged to the shambles like a beast with bandaged eyes, or whether, in full and joyous presentiment of the life which will spring forth from your sacrifice, you offer yourself freely on the altar of eternity."

If we turn from these stirring thoughts of the "Grundzüge" to the essential ideas of the "Reden," we find virtually the same mighty appeal, with this difference: in the "Grundzüge" it is an appeal to the highest and noblest in man, in the

[1] *S. W.* VII, 63.

"Reden," it is an appeal to the highest and noblest in the German. This change of front, so to speak, was a natural result of the cataclysm into which Germany meanwhile had been plunged, but it did not involve a change of heart. In the battle of Jena and its disastrous consequences Fichte saw the final collapse of the "age of absolute sinfulness," as which he had designated the present age in the "Grundzüge" of 1804–05. Now that Prussia, shorn of more than half of her territory, weighed down by fabulous amounts of war extortions and "reparations," lay helpless at the feet of the conqueror, while the rest of Germany had practically been degraded into a French province, he felt instinctively that the lowest depth level had been reached, that a new order of things, a new ascent to the summit of life was near. Now he lifted his voice once more, not to lament or condemn, but to encourage and inspire. From out of the abyss of national shame he turned the glance upward into the future of national greatness.

The image of German character, then, which Fichte traces in the "Reden" of 1807–08, is not a type that has been realized already, but a type to be realized, an ideal type of national character, which through heightening and spiritualizing of all personal effort is to become a perfect embodiment of universal humanity. With the simplicity of true greatness Fichte, in a letter written from Berlin[1] in the midst of the French occupation, has pictured the state of mind from which the "Reden" were born. "People here," he says, "are near despair, and it is hard to see what is to become of us next winter if these guests are not going to leave us. I, locked up in a lonely garden house, am guarding myself as well as I can lest a single sound of that despair, or of the insolence by which it is caused, should cross my threshhold, in order that I may retain the freedom of the spirit which will enable me to think out the principles of a better order of human affairs." Seldom has an intellectual program been worked out more fully and nobly than the program indicated in these words. For, exaggerated and historically untenable as is the unconditional glorification of German ideals of life in the "Reden," it is at the same time

[1] To Beyme, Sep. 29, 1807; *Briefwechsel* herausg. von Hans Schulz, II, 486.

an exposition of what is finest and highest in human character as such, and might with equal fitness be applied to any people, as an encouragement in a great national emergency. From beginning to end these addresses are an appeal to the inner life, to the power of each individual to reconstruct himself, to evoke a new world from the deepest recesses of his consciousness, to make himself a fit instrument of the regeneration of the social whole of which he is a part. That the system of national education which is to accomplish this radical reconstruction both of the individual and of society is based upon the pedagogic principles of Pestalozzi, is sufficient testimony to Fichte's faith in the value of these ideas for the bringing on of a higher and freer epoch of humanity at large.

Even in this mighty appeal, then, to national consciousness there are found vibrating the deep undertones of cosmopolitan feeling which distinguish German culture in its highest representatives. With the elemental force and the majestic grandeur of a Beethoven movement the all-embracing adjurations of the fourteenth address—the finale of the whole—storm in upon young and old, high and low, the living and the dead, the homeland and foreign countries, the past, the present, and the future. As if propelled by the world spirit, they arouse the deepest longing of the human heart for fulness of life, for transfiguration and perfection. In the highest sense of the word, they belong to the eternally human.

Together with Fichte, Wilhelm von Humboldt and Schleiermacher form the triad of great educators toward national strength out of the national disintegration under Napoleonic tyranny. But with them, too, there is no trace of nationalistic narrowness or fanaticism. They, too, are chiefly concerned with the universally human values embedded in nationality.

Wilhelm von Humboldt is in a way the most pronounced representative of German cosmopolitanism. His Hellenism is in a much more pregnant sense than the Hellenism of Herder or Hölderlin the result of a conscious crossing between inherited modes of thought and the intellectual products of a

foreign soil. His conception of humanity is based upon the scientific study of the most ancient civilizations and their manifestations in language, religion, and art. His ideal of German culture he has condensed into the far reaching words:[1] "To weld together into a concrete form the characteristic qualities of the ancients and the moderns—this one might call the chief mission of the German character, or perhaps better the goal the attainment of which every one who wishes for a genuine ennobling of our national character must, on his part, strive to bring about." With Humboldt, then, no less than with Herder, Kant, Goethe, Schiller, and Fichte, the great task is the heightening of the national type, the striving toward the ideal of complete humanity. By assimilating certain thoughts of Schelling's philosophy of nature, however, he gives to this striving a still wider background, an inner connection with the cosmic process of the development of the conscious from the unconscious, of spirit from nature, and thereby with the fundamental unity of nature and spirit underlying the whole variety of external forms. The rise toward a national type which is to unite the spirit of the ancient world, bent upon the finite, with the spirit of the modern world, bent upon the infinite, thus becomes for Humboldt a climax in the development of universal life. And the individual man who has succeeded in accomplishing this unification of polar contrasts in himself appears to him as charmed against all the adversities and disasters of actual existence. For he has reached the point, to use Humboldt's own words,[2] "where all human affairs show their true form and yet do not frighten or shock any more, where pain itself through its formative depth comes to be a fruitful exercise of the mind, where from perfect harmony with one's self there springs an invincible inner serenity, and where cold, iron necessity itself appears only as a power through the impact with which one completes himself and his destiny."

While the blending of Hellenism and Teutonism was the

[1] Letter to F. A. Wolf (125) of Aug. 20, 1797; quoted by E. Spranger, *W. v. H. und die Humanitätsidee*, 262.

[2] Letter to Caroline v. H., quoted by Spranger, *l. c.* 196.

soil from which Wilhelm von Humboldt hoped there would blossom forth a higher national culture, Schleiermacher's vision of a higher national morality arose from the longing for a truly human, all-embracing religion. To be sure, the "natural" religion of the age of Enlightenment had already proclaimed in a way a gospel of universal humanity; but this religion of reason had after all remained something decidedly vague and shadowy. Only through Schleiermacher's insistence upon feeling and intuition as its fundamental essence, religion became once more a power setting in motion and inspiring the totality of man's instincts and ideals. Through feeling himself as an organic part of an infinite world whose forces permeate his whole existence and upon which he reacts with every pulse beat of his own being, man acquires the capacity of truly living in the intuition of the divine, of seeing the eternal in everything temporal, the infinite in everything finite, and of thereby raising his own personality to a condensed type of humanity at large, and of making it a vessel of humanity's highest tasks.

This conception of the world as a system of endless spiritual forces, born from Schleiermacher's own personal longings and experiences, became the foundation of his imperturbable trust in the inner soundness and the high destiny of German national life. It supported him in his efforts to create in the growing generation a state of mind wholly imbued with the certainty of the eternal, seeing in the country's present affliction only an incentive for reviving all permanent and productive impulses, ready to place all individual lives into the free but unconditional service for the preservation, the expansion, the spiritualization of the life of the whole. In the days after Jena, this certainty of the eternal forced upon his lips the words:[1] "To have a new Germany, the old Germany probably will have to be torn into still further fragments, much more even than now." After the occupation of Halle by the victorious French and the dissolution of the University, this certainty gave him strength to write:[2] "I for my part am

[1] Letter to Friedrich von Raumer, Jan. 12, 1807; *Aus Schleiermachers Leben* IV, 132.

[2] Letter to Brinckmann, Dec. 22, 1806; *ibid.*, 128.

firmly resolved, as long as I can scrape together in Halle what I need of potatoes and salt, to stay here and await Germany's fate here, whether perhaps a resurrection of Halle is brought about which would satisfy me." And all the sermons through which in the years preceding the Wars of Liberation, as pastor of the Dreifaltigkeits Kirche in Berlin, he stirred the intellectual élite of the capital into new courage and energy, are tuned to the same key; they all preach in ever new variations and applications the fundamental creed of the "Reden über die Religion" and the "Monologen": the belief in the creative power of the supersensual; the idea of the individual as something absolutely unique, free, and imperishable; the conception of humanity as a spiritual universe in which from an infinite number of individual forms, varieties, contrasts, from constant struggles, revolutions, and upheavals, there arises a grand unity of development, of ever repeated approaches toward a higher type.

It is clear, then, that Schleiermacher, as well as Humboldt and Fichte, were concerned not so much with the immediate and circumscribed questions of political and material recovery as with the needs of the invisible church of the German spirit, whose confines coincide with the wide realm of human ideals of all ages and nations.

That it was these three men who in the years following the catastrophe of 1806 contributed above all others to the moral and intellectual rebirth of the German people, by the founding of the University of Berlin and the reorganization of the whole Prussian system of higher education, is a splendid testimony to the noble cosmopolitan spirit in which the work of delivering Germany from foreign oppression was undertaken. These men may be hailed as champions and patron saints of the present generation of thinkers and progressive statesmen who, from a Germany defeated, humiliated, disarmed, and held in bondage, are building up a new Germany based upon the belief in the ultimate victory of freedom, humanity, and international good will.

THE IDIOM OF JUTLAND

By JOHN DYNELEY PRINCE

(*Read April 28, 1927*)

JUTLAND, probably the home of the ancient *Íotas* of Anglo-Saxon history, is the continental part of modern Denmark, extending from the present frontier at Flensborg northward to the Skàw (*Skagen*), and embracing several islands, as, for example, Als on the east and the formerly Frisian-speaking Fanö and several other isles on the west.

The exact connection of the ancient Jutes and their possible identification with the Angles have long been matters of discussion for scholars. It would seem from the Anglo-Saxon record that it was the ancestors of the modern tribe of Danish Jutes who settled in Kent and on the Isle of Wight in England. According to the Anglo-Saxon accounts, the British king in 449 A.D. sent for certain Germanic mercenaries to aid him in his struggles with the Picts: *thá cómon thá menn of thrym maéghdhum Germanie; of Eald-Seaxum; of Anglum, of Íotum. Of Íotum, cómon Cantware and Wihtware, thaet ys, séo maéghdh the eardath on Wiht and thaet cynn on West-Seaxum the man nú ghýt haét 'Íutna cyn'*; "then came the men of three tribes of Germany; of the Old Saxons, the Angles (and) of the Jutes; of the Jutes came the Kentish people and the Wight people; that is, that tribe which now lives on Wight and are kin to the West Saxons whom they still call 'Jute-kin.'" [1]

[1] "Easy Readings in Anglo-Saxon Prose," W. A. Craigie, Edinburgh, 1925. Jutland was known to the Ancients as the Cimbric Chersonnesus. The theory that the Cimbri were a Celtic tribe (akin to the modern Welsh Cymry), who invaded and occupied Jutland at an early date, on account of the amber trade, has recently been advocated by the well known Norwegian scholar, Alexander Bugge, in a learned and interesting treatise in the *Nordisk Tidskrift*. Prof. Bugge believes that the Celtic influence was strong at one time in Denmark generally, but was subsequently sub-

It would appear from this statement that the Anglo-Saxon chronicler regarded the Angles as distinct from the Jutes, particularly as he adds: "of the Angles (who since then have remained west between the Jutes and the Saxons) came East Angles, Middle Angles, Mercians and all the Northumbrians."

It would seem clear, however, for the following reasons that the ancient Jutes and Angles were allied to each other in speech and race. The ancient dialect of Kent occupies a place immediately between the Anglian and West Saxon. In fact, the early Kentish seems more closely allied to Mercian, which was an Angle dialect. The later Kentish, however, was inclined to conform to West Saxon characteristics, although it always kept a certain kinship with the Angle Mercian.

It would appear, therefore, that the Jutes and Angles, although perhaps primitively of common origin, were, at any rate, separated clans as early as the time of their first arrival in Britain in the fifth Christian century.

Modern Danish Juttish constitutes the west Danish group of dialects. The other groups are, first; Island Danish, comprising the idioms of North and South Sjaelland, the Lolland-Falster subgroup, and the dialects of Langeland and Fyn. The latter dialects, although so close geographically to Jutland, differ in a marked manner from the Juttish speech. Secondly in this connection, we have the East Danish group of idioms, notably the Bornholmsk and the Scandian (the Danish dialect of Skaane) which gradually merge into real Swedish on the north.[1]

The object of the following treatise, which completes the descriptive linguistic work done by the writer while he was Minister of the United States to Denmark (1921–1926), is to describe briefly by illustrations and exposition the modern idiom of Jutland.

There are four distinct dialects, or better, dialectic groups

merged by the prevailing Germanic stock. This idea, although by no means proved, is certainly worthy of consideration (see my article in Danish, "What the World owes the Celts," in Gads *Danske Magasin*, Copenhagen, March, 1926, pp. 129–128).

[1] "The Danish Dialect of Bornholm," by J. D. Prince, *Proc. Amer. Philos. Soc.*, p. 191, for the classification of Danish dialects.

within the territory embraced by Jutland and its islands; viz., the northern group, which is spoken north of the Limfjord; the eastern idioms, which extend approximately on the east coast south from the Limfjord down to the former Danish frontier near Kolding; the western dialects, extending from the western end of the Limfjord to a point slightly south of the former Danish border; and the southern idioms, which comprise all the purely Danish *patois* spoken south of the former frontier, just mentioned, down past the present German frontier, and existing also in various districts in the present German Slesvig, separated, however, by communities speaking the Slesvig *Plattdeutsch* and, here and there, by Frisian speaking districts.[1]

Of all the speech-variants of Denmark, Juttish is unquestionably the harshest, because, as will appear below, it abounds with the strident glottal catch peculiar also, though to a less extent, to Standard Danish. The Jutes ejaculate this catch, not only much more frequently, but also much more perceptibly than do the ordinary Danes. Even persons educated in SD., who can no longer speak Juttish proper, and who have resided long years away from Jutland, are easily to be detected by the vehemence of their catch in Standard Danish. This striking sound, which is not heard at all in Bornholmsk or Scandian, and rarely in Island Danish, is used in Juttish and in SD. either to replace the ancient cadence common, for example, in Swedish, or to compensate for contractions. It is noticeable, that of the Island Danish idioms, those of Lolland-Falster, Langeland and Fyn show no catch at all, while South Sjaelland has it but rarely. In North Sjaelland, it is occasionally heard and, of course, it is very common and even necessary for proper diction in SD., which is a refined development of the city dialect of Copenhagen.

The Jutes are proud of their idiom, which they have preserved, in spite of the Danish Government schools. Of late years, Juttish has even enjoyed a certain literary position, the inauguration of which was the celebrated work of Sten Stensen Blicher, *E Bindstouw* (see below), which did for Juttish what

[1] *Kort over de Danske Folkemaal*, 2 vols., Copenhagen (Gyldendal), 1912

"Tim Bobbin" of Rochdale did for the Lancashire English dialect; that is, the Juttish idiom has now achieved a certain dignity and is looked upon as a fitting vehicle for some excellent poetry, as well as for admirable prose sketches. There is, however, no concerted movement on behalf of the language, such as appears, for example, on the Faroes and, to a less extent, on Bornholm, to reduce the idiom to a uniform "language" and thus stimulate a Juttish "nationality." It is true, however, that every Jute (like every Scot) will tell the inquirer that he comes from the "backbone" of the country and that the Jutes are the "best people."

There can be little doubt that Juttish will live for a long time to come, in spite of Copenhagen contempt for its "Doric" vocables. In I. Jacobsen's forceful little book "The Brickkiln Gatherings" [1] and in the really charming works of Anton Berntsen,[2] Juttish is alive now quite as vividly as is the Lowland Scottish in Britain.

In the following sketch, analysis has been made only of the West Juttish and the Eastern Juttish idioms, as being the most characteristic. The writer could unfortunately get but little material for the dialect north of the Limfjord, while the southern dialects are very Germanized and, therefore, not so illustrative of true Juttish speech.

The writer takes this opportunity to thank Captain Svend Reffs of Copenhagen, Jaegermester Trode Neergaard of Tirsbaek Slot, near Vejle in East Jutland, Mr. Sigurd Andersen of Oxholm Slot in North Jutland, as well as Mrs. Eric A. Langberg, formerly of Kolding, for many valuable suggestions.

Phonetics [3]

The phonetics of Juttish are difficult, chiefly because they are so different in the various parts of the Peninsula. The

[1] *Taejjelownssjow* "The Brickkiln Entertainment," Aarhus, 1892.

[2] By Anton Berntsen: *Niels Vaewwers Rejs* 'Niels Vaewwer's Trip,' Vejle, 1919; *Skowwer* 'Burnt Porridge,' Vejle, 1923; *Gjemmaewel* 'Stored Apples,' Copenhagen, 1921; *Megstur* (poems), Copenhagen, 1925. Note that *Skowwer* = burnt porridge, adhering to the bottom of a pot.

[3] The following abbreviations have been used: D = Danish; EJ. = East Juttish; J. = Juttish; OD. = Old Danish; SD. = Standard Danish; SJ. = South Juttish; WJ. = West Juttish.

standard work on the subject is the so-called language map of Valdemar Benneke and Marius Kristensen, published at Copenhagen (*Gyllendalske Boghandel*), 1898–1912, which purports to give the grammatical and sound variants according to districts; a herculean task and one which has been wonderfully well accomplished. The chief fault with the work is perhaps that there is no *résumé*, by means of which the student can get a *general* view of the variations. One is also tempted to doubt the absolute accuracy of the geographical lines laid down, on either side of which certain sounds occur or do not occur.

Juttish does not labor under the same difficulty as Bornholmsk as to the usual method of reproducing the dialects. In fact, all authors in this idiom follow essentially the same system of notation with only slight deviations. This system is indicated as follows, in order to enable the reader to get, at least, an approximate valuation of the extracts given below. Generally speaking, the following values are observed: *a* = *a* in 'father' or *o* in 'not'; *aa* = short *aw* in 'awful'; *ae* = *a* in 'hat'; *aeae* = the same sound prolonged; *aw* = *ow* in 'how'; *e* = *e* in 'met', or *a* in *cane* (this sound I indicate by ê); *ej* = *ä* (in 'hat') + short *i*; *i* = *i* in 'pin'; *ie* = *ee-ŭh*; *o* = pure *oh*, without foll. *u*-sound; *oe* usually = *o'e*, with glottal catch; *ö* (or *ø*) = *i* in 'sir' (short); *öö* = the same sound prolonged; *öj* as *ir* in the New York City pronunciation of 'thirty'; *ow* = *oh* + short *u*; *u* = *oo* in 'foot'; *uu* = *oo* in 'boot'; *y* = French *u*; *yw* = French *u* + short *ŭh* as diphthong.

The consonants require less comment: final *-d* after a vowel usually = *th* in 'there'; final *-d* after *l* serves as a glottal catch. *H* before vowels is the simple aspirate; *h* before *w* is a rougher aspirate in the west, as *hwa* = *hŭ-wáh* 'what.' *H* following a vowel was used by the older authors to indicate the glottal catch (') and also a long vowel, as *aahn* = SD. *anden* 'other,' pr. *aw'n* (*aw* in 'awful') and also *awn*. *J* is the consonantal *y*; but after vowels = the *i*-diphthong; as *aej* = *ay* + *i* (short) or *ä* (as in 'hat') + *i* (short).

The omnipresent glottal catch[1] is usually indicated by doubled consonants especially *-kk*, and *-tt*. This is, however, not the case with *ett* = SD. *ikke* 'not,' nor with all the combinations in *-ss* and *-dd*; thus, *aasse* = SD. *ogsaa* 'also' is pronounced *osse*, and not *o'se*; cf. also *udden* 'outside.' *Gj* is pronounced *g͡y*; rarely as *d͡y*.

Kj is usually pronounced, especially in WJ., like the palatalized Polish *ć* (*tsy*), a soft *ch* as in 'rich,' not as in 'church.' In EJ., however, *kj* is the palatalized *k*.

Ng = *ng* in 'singer,' but *-nng* = *ng* in 'finger,' even at the close of a word.

W = Eng. *w*, a characteristic of Juttish, instead of the Dan. labial *v*, but final *-ww* has a glottal catch before the close of the diphthong.

The catch has, generally speaking, taken the place of longer forms, such as the SD. Inf. ending *-e*; thus, for SD. *drikke* 'drink,' J. usually has *drekk* (*dre'k*); for SD. *taenke* 'think,' *tae'nk*, etc.

The following brief table of correspondences between J. and SD. will suffice to show the great phonetic differences which exist between this dialect and the idioms of the rest of Denmark. It should be remarked, that these differences are of irregular occurrence and are not to be taken as a rule.

J. *a*	may correspond to			SD, *e*	*gjahn—gerne* 'willingly'
aa	"	"	"	*a*	*taale—tale* 'speak'
	"	"	"		*haaer—har* 'has'
	"	"	"		*saah—sagde* 'said,' etc.,
ae	"	"	"	*e*	*daete—dertil* 'thereto'
aej	"	"	"	*e*	*baejst—be(d)st* 'best'
aw	"	"	"	*af*	*awten—aften* 'evening'
e	"	"	"	*i*	*te—til* 'to'
i	"	"	"	*u*	*sgi* (WJ.)—*sgu* 'indeed'
ie	"	"	"	*e*	*hiel—hel* 'whole'
ie	"	"	"	*ae*	*nies* (WJ.)—*naes* 'nose'
ue	"	"	"	*o*	*skuel* (WJ.)—*skole* 'school'
	"	"	"		*suet* (WJ.)—*sort* 'black'
o	"	"	"	*ō*	*hoer—hörde* 'heard'
ow	"	"	"	*o*	*jow—jo* 'yes' (after neg.)
ō	"	"	"	*y*	*rōg—ryg*
ōj	"	"	"	*aj*	*mōj—meget* (pr. *majjed*) 'much, very'

[1] The glottal catch (*Stödtone*), peculiar to languages as far apart as Danish, Lettish and Arabic, is a voice-stop, almost identical with the sound of the Arabic *Ayin*.

ōj	"	"	"	*y*	*bōj* (old WJ.)—*by* 'town'
ue	"	"	"	*a*	*vuet* (WJ.)—*varede* 'lasted'
	"	"	"		*suet* (WJ.)—*svarede* 'answered'
u	"	"	"	*o*	*suet*—*sort* 'black'
f	"	"	"	*b*	*dyft*—*dybt* 'deeply'

The following contractions are of interest: J. *noed; nowwed; nöd* (WJ.) for *Noget* 'something'; *aale-aldrig* 'never'; *saah* (WJ.)-*sagde* 'said.' Omissions of consonants are also common, as, *te* for SD. *til; ae*, for *er; dae* for *der; ka* for *kan; ku* for *kunde*, etc.

Note finally here, that WJ. *ejjes* = SD. *ellers* 'otherwise,' showing the well known phenomenon of the palatalization (*Mouillierung*) of soft *e* to *y*, which occurs so frequently, for example, in the Spanish dialects of South America: *ayyá* for *allá*; *hayyár* for *hallar*, etc. Cf. below in I., *hejer* = *heller*.

Examples

The following three extracts which embody the distinctively WJ. older dialect of Sten Stensen Blicher's famous collection known as *E Bindstouw*, an idiom which no longer exists in its former purity, as well as the EJ. of the region of Vejle Fjord should suffice to illustrate generally the character of the modern language.

I. E Bindstouw, 'The Knitting Bee,' Sten Stensen Blicher, København, 1895, pp. 11 ff.

Jösses, hwo skrannied di aa de haear histaarri . . . undtawn Kjen Paejster. Hun wa saa mösk. A trowr knap, Én[1] kun ha slawn et Smiel aa hin mae en Wownkjaep. "Do aer aalti saa fos" saah Mowns te hin, "Hwa skaae Do?"—"A skaae slet et Ant" saah hun, "end no wil A te aa kwehd Jer en aahn Vihs. Begynner en let raeèle, saa foer en Forbaejring faa de i ae Aehn. Aa hwa de sto skröwwen i en, de ae sann, faa de skidh i den Böj, hvor A ae födh, aa de ae hejer ett laenger sihn end te mi Baejstemuer ku granngivvele haaww ed." Daehmae stinned Kjesten hinne Rög aa saang:

Po tréj Bién húmper ae Háelhaejst astéj
Faa dé te han haaer ikke flíer:
Men hwém dae ska möhd ham epó si Véj
Humper snáaer i ae Kjerrgor níer.

[1] *En* = Dan. *man*; French *on* = the indef. 'one.'

Aa Mowns han wá sae saa lōste en Sve'nd
Hwor hán hor Fiolen han dánnst daer
Han hád sae no aasse en fúldtrow Ven
Aa di wa hin Métt Mari Hánster.

Lord (Jesus!), how they roared with laughter at this here story . . . except Kirsten Pedersdatter. She was so cross. I hardly think one could have knocked a smile out of her with a wagon-pole. "You are always so cross-grained," said Mogens to her, "what ails you?" "Nothing else ails me," said she, "but that now I intend to sing you another song. If it begins a little terribly, there will be improvement towards the end. And what stands written in it, that is true, because it happened in the town where I was born and that also not longer ago than that my grandmother could accurately remember it." Thereupon Kirsten straightened up her back and sang:

On three legs limps the stallion away
Because he has no more;
But he who may meet him upon his road
Will quickly limp down to the churchyard.
And Mogens he was so lusty a swain
Wherever he heard the fiddle he danced there.
He had for himself also a faithful (girl) friend
And that was she—Mette-Marie-Hansdatter.

Phonetics and Grammar

Skrannied = SD. *skrattede*; note the ending *-ed* for *-de*. *Di* = 'they'; *aa* = SD. *af* or *paa* 'at' and *de* is the demonstrative, here for *den*. *Undtawn* = SD. *undtagen*; note the absence of the *-g-* and the consequent broadening of the vowel *-aw-*.

All proper names suffer shortening; note here *Kjen* (pr. *Tsjen*) *Paejster* for *Kirsten Pedersdatter*. The Jutes even now adhere to the old northern custom of using the fem. form in patronymics.

Mösk = Sw. and Norw. *morsk* 'cross.' *Trowr* (EJ.. *truer*), for SD. *tror*. *Aa hin* = SD. *paa hende*. *Wown*, for SD. *Vogn*, a similar change to that seen above in *slawn* = SD. *slagen*, now not in use as verbal participle with the auxiliary *have* which is always used with *slaaet; kunde have slaaet* 'could have struck.' Note the use of *kjaep* (pr. *tsjaep*) 'staff,' pole,' in place of SD. *Stange*, *Kjaep* is dialectic in D., but the usual word in Sw. for a stick or staff.

Fos 'contrary' = SD. *modsat*. *Saah* 'said' = SD. sagde. *Skaae* = SD. *skader*; in J. used subjectively = 'be ailing.' *Slet et Ant* = SD. *slet ikke Andet*; note the common J. *et(ett)* = SD. *ikke* 'not.' 'I am going to' is vividly expressed by *a vil te aa* = SD. *jeg vil til at* (not used). Note *kvehd* (without catch) = Sw. and OD *kvaede*. *Aahn* without glottal catch for SD. *anden*. Observe *Vihs*, for SD. *Vise* 'song' and *rae'ele* = SD. *raeddelig* 'terribly,' with omission of the *-dd-* and catch. *Forbaejring*; omission of *-d-* for SD. *Forbedring* (improvement. *Ae Aehn* 'the end' = SD. *Enden*; note catch, instead of *e*-ending in SD. *Ende*.

Aa here = and = SD. *og* (pr. *au*); note that *aa* in J. may mean 'and' = SD. *og*, *paa* 'upon, on,' and the verbal prefix *at*, as above, *te aa* = SD. *til at*. *Skröwwen* = SD. *skreven*. *I en* 'in it' = SD. *i den*. *Ae sann* 'is true,' with absence of neuter *-t*, as SD. *sandt*. *Skidh* = SD. *skēde* 'happened.' *Böj*, not much in use to-day, for SD. *By* 'town.' *Hejer ett* with *-ej-* for *-ell-* = SD. *heller*[1] *ikke* 'also not.' *Sihn*, without catch, = SD. *siden*. *Te* is the common J. conj. = SD. *at* 'that.' Note poss. *mi* 'my,' used for all genders and for sg. and pl. *Granngivele*, adv. *grann* 'exact' + *giv* 'giving' + adverbial *-ele*. This form is unknown in SD. *Haaw* 'remember' usually *haws* (EJ.) = OD. *hue*, for *haa* (*u*) *g*; cf. Sw. *haag*. *Hukomme* 'remember' is still used in SD. and *komma i haag* in Sw. *Ed* (pr. *edh*). is the unstressed form of *det*, sometimes = *et* in J. *Stinnede* = SD. *rette* 'straighten.' *Rög* = SD. *Ryg* 'back.' *Saang* = SD. *sang* 'sang.'

Po 'on' is used here instead of the usual J. *aa* = SD. *paa* 'on.' *Bien* = SD. *Bēn* 'leg.' *Haelhaejst* = SD. *Hingst*; lit. *Hēlhĕst* 'whole horse'; 'stallion.' *Astej* = SD. *afsted* 'from the place' = 'away.' This *-ej* is pronounced *äj*, or even *ääj*.

Faa de = SD. *fordi* 'because.' *Haaer* = SD. *har*. *Ikke* is used here, instead of J. *ett*, probably for the metre's sake. *Flier* = SD. *flere* 'more.' The interchange of *ie* and *e(ee)* is common (see table above). Note *Bien* = Bēn, above.

[1] Cf. *ejjes* and *ellers* 'otherwise,' above.

Möhd, = SD. *möde* 'meet.' *Epo* = SD. *oppaa* (old). *Si*[1] here is for *hans* 'his (way)'; indelcinable. *Kjerrgor* (pr. Tsjergor) = SD. *Kirkegaard* 'churchyard.' 'Church' in WJ. is *tsjerk* and *tsjertsj*, almost = Eng. *church*; thus, *A wil gaa up te ae tsjertsj* has almost the sound of broad Yorkshire dialect.

Mowns = (pr. Mauns) = SD. *Mogens*. *Han wa sae* 'he was for himself' (reflex. dat.). *Löste* = SD. *lystig* 'lusty, merry.' *Swend* = SD. *Svend* = Eng 'swain.' *Hor* is the imperfect = SD. *hörde*. The J. pres. would be *höör*. *Dannst*, imperf. = Eng. 'danced' = SD. *dansede*. *Han had sae* 'he had for himself,' as above: *han wa sae*. *Aasse* = SD. *ogsaa* (pr. *osso*). *Ven* here means 'girl friend.' *Di* 'that one,' is emphatic for SD. *den der*. *Hin Mett Mari Hanster* 'she Mette Marie Hansdatter; note the use of the personal pron. to indicate a name, common in dialectic Scandinavian everywhere, as *Han Jens* 'he Jens'; *hun Hilda* 'she Hilda.' This is now regarded as vulgar in the standard languages. It is a usage somewhat similar to the German vulgar style: *der Johan, die Anna*, with the def. article.

II. En Skidt Knaejt,[2] from Skowwer, A. Berntsen, Vejle, 1923, pp. 14–15.

Lisbet. *Kom lii hieud; A haaer en Dram te Dae.*

Sören. *Naej, A ska ingen Dram ha i Daw.*

Lisbet. *Jow vest, ska Do ha en; A haae skenket en te Dae.*

Sören. *De ae snaae en Skam, den ett ska drekkes.*

(Lisbet gaaer ud aa lukker ae *Daar*) *Aa-aa, luk op faa mae; Lisbet haae laaset mae ind i ae Kjökken! A ka ett kom ud!*

Mads (*frae si Seng*) *Aa Din Taask, hwa vild Do dieud atte? Lae Lisbet luk op faa Dae igjen.*

Sören. *Hun ae gawwen. A ae jenn heruud. Aa ae Daar ae laaset.*

Mads. *Bi let; No ska A kom aa luk Dae uud. Aa-aa mi Maww; A ka snaaer ett röör mae faa Piin.* (Han höör Nowwen kom ind i ae Huu's) *Hwa Skam, hwem komme no?*

Kaare-Stine (kommer ind) *Godaw i ae Stoww! No, de ae nok ett godt mae Dae i Daw, Mads. A snakket mae Lisbet lii udden ae Goer; aa hun vild ha, A skuld go ind aa si te Dae: Hudden haae Do et?*

[1] Not reflexive.

[2] 'A bad fellow.'

Lisbet. Come right outside; I have a dram for you.

Sören. No, I shall have no dram to-day.

Lisbet. But yes, certainly you shall have one; I have poured one out for you.

Sören. It is surely (lit. 'soon') a shame, that it should not be drunk. (Lisbet goes out and shuts the door.) Oh, open for me; Lisbet has locked me in the kitchen. I cannot come out!

Mads (from his bed). O you fool, what would you (do) out there anyway. Let Lisbet open for you again.

Sören. She is gone. I am alone out here. And the door is locked.

Mads. Wait a little; I will come now and let you out. Oh, my stomach. I can scarcely move for the pain! (He hears someone come into the house.) What a nuisance! Who is coming now?

Kaare-Stinne (comes in). Good day in the room! Well, it is not well with you to-day, Mads. I was talking with Lisbet just outside the farm; and she would have (it) that I should go in and say to you: 'How are you?'

Lii = SD. *lige* 'just'; here = 'right' (outside)'. *Hieud* = SD. *herud*. *A haaer* 'I have'; note the present *-r*-form here, but just below: *a haae*. *Te* = SD. *til*. *Daw* = SD. *Dag*; *i Dag* 'to-day.' *Jow vest* = SD. *jo vist*; the adversative 'yes,' used after a negative. *En Skam den ett ska drekkes* 'a shame that it shall not be drunk'; note the absence of the conj. *te* = SD. *at* 'that.' *Kjökken*, pr. *Tsjö'ken* or *kyökken*. *Ett*, as in WJ. for SD. *ikke* 'not.' *Atte* = SD. *atter*; lit. 'once more,' like Germ. *noch ein Mal*; corresponds to 'anyhow' here.

Gawwen 'gone'; cf. WJ. *gon*, above = SD. old form *gangen*. In SD. *hun er gaaet* 'she is (has) gone.' *Jenn* = SD. *alene* 'alone.' *Daar* 'door' = SD. *Dör*. *Bi let* = SD. *vent lidt* 'wait a little'; J. *bi* = Eng. 'bide.' *No* = SD. *nu*, 'now.' *Nowwen* = SD. *Nogen* 'someone.' *Godaw* (one word) = SD. *God Dag*, pr. *Godáe* 'Good Day.' *Stoww* (the *-ww* shows catch) = SD. *Stue* = Sw. *stuga*, Germ. *Stube* 'room.' Note: *hun vild ha A skuld go ind* 'she wanted I should go in' = 'wanted me to go in'; this construction still prevails in the American New England *patois*. *Hudden* = SD. *hvordan* 'how'? = 'How hast Thou it?'

III. GJEMM AEVVEL,[1] ANTON BERNTSEN, KOBENHAVN, 1923, P. 24.

Ywl[2]

A goe haehjemm aa ae saa kyw,
Mi Mand ae dö aa henn
de ae saa möj et ussel Lyw,
faa Kwinndfolk aa vaer jenn.

Nok wa mi Mand ett swaae klog,
aa dutt ett te raet möj.
Han wa en grimme dowwen Slog
Mi Paeaeng dem faek han öj.

Han wa et Höwwed te aa skro
De drevlet i hans Skekk.
Ve Gilder skuld A nöww pas o
Hwomöj han faek aa draekk.

Di mindt te A wa baejje bat,
Ve han gik hen aa döö;
No haaer A kons wo gammel Kat
aa saa mae sjel aa föö.

Men A ka maerk, no han ae henn,
A howwet ham ilywl.
De ae saa strenng aa go aaldjenn
Aa taenk o, de ae Ywl.

CHRISTMAS

I go about here at home and am so sad.
My husband is dead and away (hence).
It is so very miserable a life for womenfolk, to be alone.

Yet, my husband was not very clever
And did not amount to very much.
He was an ugly stupid churl
(And) wasted my money.

He was a great one to chew tobacco.
It dribbled down his beard.
At meetings, I had to watch carefully
How much he got to drink.

[1] 'Stored Apples'; those kept for future use.
[2] 'Christmas.'

They thought that I was better off,
When he went and died;
Now I have only our old cat
And, of course, myself to feed.

But I can feel, now that he is away,
I liked him all the same.
It is so hard to go all alone
And think that this is Christmas.

A goe 'I go'; note absence of the pres. *-r*. *Kyw* 'sorrowful' = SD. *kēd*. *Mi* 'my'; used for all genders and numbers. *Dõ* 'dead' = SD. *död*. *Henn* 'hence, away' SD. henne. *Möj* 'very, much' = SD. *meget* (usually pr. ma*jjedh*). Note absence of the neuter *-t* in *ussel* before *Lyw* = SD. *Liv* 'life' (neut). *Jenn* 'alone' = SD. *alene*. *Jenn* really means 'one' and this usage has a resemblance to Russian *odín* 'one' and 'alone.'

Wa 'was' = SD. *var*. *Ett* 'not' = SD. *ikke*. *Swaae* 'very' = SD. *saare*. *Dutt* 'amount to something' = SD. *duede*. *Dowwen* 'stupid and lazy' = SD. *doven*. *Slog* 'churl, unattractive fellow.'[1] *Mi paeaeng* 'my money' = SD. *mine Penge* (pl.). *j* 'waste' = SD. *õde* (*laegge*).

Höwwed 'great fellow, great one.' *Skro* 'chew tobacco' = SD. *Skraa* 'plug of tobacco,' but SD. *tygge* = 'chew.' *Drevlet* 'dribbled' = SD. *dryppe*. *Skekk* 'beard' = SD. *Skaeg*. *Gilder* are meetings at the public house, where the peasantry drink and carouse. *Nöww* 'accurately' = SD. *nöiagtigt*. *Pas o* 'watch' = SD. *passe pea*. *Hwomöj* 'how much' = SD. *hvormeget*.

Di mindt 'they opined' = SD. *de syntes*; *de mente*. *Baejje bat* 'better off' = SD. *bedre stillet*. *Ve* 'when'; prob. cognate with Eng. 'when' = SD. *naar*, or *da*, with the past tense. *Döö* 'died,' for SD. *döde*, but J. *döö* is really cognate with the strong form as in Sw. *dog*. In SD. *dõ* 'to die' has become weak., as, in Eng. 'died. On the other hand, *döö* here may be the infin. = SD. *dö*; *aa döö* 'to die.' *Kons* 'only' SD. *kun*(*s*). *Wo* 'our' = SD. *vor*. *Gammel Kat*

[1] Probably cognate with Eng. 'slug.'

'old cat'; note the absence of the def. *-e* (*gamle*). *Föö* 'to feed' = SD. *föde*.

Howwet 'liked' = SD. *holdt af*. *Ilywl* 'all the same, anyway' = SD. *alligevel* (pr. *alijevel*). *Strenng* 'hard'; note the *-nng* = Eng. *ng* in 'finger'. In SD., *streng* means 'severe.' *Aaldjenn* 'all alone' = SD. *alene*. *Ywl* 'Christmas, Yule = SD. *Jūl*.

Grammatical Sketch [1]

The Article

Since the days of the Vikings, the majority of the Scandinavian dialects have used the suffixed article for the definite form; as Swed. *mannen*, Dano-Norwegian *Manden* 'the man,' but the greater part of Jutland forms a notable exception to this rule. Thus, in west and south Jutland, the definite article *ae* (*e*) is prefixed as in English, German and Dutch; *ae man* 'the man'; *ae kuen* 'the woman'; *ae baa'n* (or *baa'r*) 'the child.' In parts of east Jutland, on the other hand, the definite article is not only suffixed as in standard Danish, but in some places a distinction is made between the two genders; common and neuter, as *man'ĕn*, *kuoněn*, but *baa'nĕt* (neut.).

The indefinite article is universally prefixed; *en* for common gender and prefixed *et* for neuter.

It will be observed that the western Juttish definite article is *ae* (ĕ, e, ē) for all genders, except that in variants; viz., the dialects of Gudenaasöerna and Bjaerge, the article is *a*. Before words beginning with a vowel, this WJ. *ae* usually changes to *aer* (= *aedh* in some places). In Vensyssel, however, the article with the masc. and fem. sg. and common plural of nouns ending in *l*, *n*, and (masc.) *j*, is contracted and equalized, as follows:

Masc. Indef.	*Masc. with Def. Article*
en ûl 'wolf'	*ul'* 'the wolves'
en nē'jl 'finger-nail'	*nēl'* etc.
en sdi'n 'stone'	*sdjēn'*

Plural Indef. and Def.		*Fem. Indef. and Def.*	
ûwl 'wolves'	*ul* 'the wolves'	*en kuen* 'woman'	*kwon'* 'the woman'
nējl	*nējl'*	*en sdjan* 'star'	*sdjan* 'the stones'
sdi'n	*sdjēn'*		

[1] In this grammatical sketch, use has been made of the language map, mentioned in Note 3 above, in addition to the writer's personal observation.

Contractions of this sort are numerous and make the dialect very difficult to understand, when spoken rapidly, as it usually is. It should be added, that differentiation of gender, as on Bornholm,[1] exist in the north-east of Vensyssel and also on the islands of Anholt and Samsö, etc. This differentiation is shown by the form of the indefinite article, the ending of the definite article, the form of the numeral *ēn* 'one' and of *mīn*, *dīn*, *sīn*, the possessive pronouns.

Indef. Article	*Def. Article*	*Numeral One*	*Possessives*
Masc.....*i*	*-i*	*jen'j*[2]	*men'j*[2]
Fem.....*aen*	*-ĕn, -n* (after vowels)	*jaen'*	*min'*
Neut.....*e, ĕ, aa*	*-ĕdh, -ĕ, -t*	*jet*	*met*

On the continent, the three genders are thus indicated in only a few districts near the Limfjord.

Although, as remarked above, no gender distinction in general exists in WJ., there is a denominative neuter distinction applied to collective nouns and to adjectives used as collectives. This gender is indicated only by the demonstrative pronoun, when it refers to such nouns. The pronoun is then neuter = *det*, only in such a case, but is *den* (common) for all other nouns, even though these nouns may have the neuter gender in the standard language. Thus, *korn* 'grain'; *byg* 'barley'; *ost* 'cheese'; *maelk* 'milk,' etc., being collectives, are referred to by the neuter *det*, while *bēn* 'bone, leg'; *hūs* 'house'; *hjaerte* 'heart,' all neuter in the standard language, are referred to by the demonstrative *den* (common).

South of Horsens, the names of young animals are treated as neuters, a phenomenon which has been observed in SJ. and in the southern part of north Jutland. Thus, *kalv* 'calf'; *gris* 'young pig'; *killing* 'kitten,' etc., are all neuters in these dialects.

Stress should also be laid upon the fact that in Juttish generally, the genders of nouns differ from those seen in standard Danish; thus, in J. *laas* 'lock'; *lyd* 'sound'; *lyng* 'heather'; *rede* 'nest' are all neuter, but common in SD.,

[1] Cf. reference in Note 2, above.

[2] This is a palatalized nasal.

while in J. *sõm* 'nail' is masc. and neuter in SD., etc.[1] In fact, the neuter is a favorite in J., a tendency which is probably a step towards the elimination of gender distinction generally.

The plural of nouns is formed in J. corresponding to the three ways of forming the plural in SD., as follows: for D. plurals in *-er*, we usually find J. *-ĕ(r)*; for D. plurals in *-e*, J. usually has the plural indicated by an accent shift; and finally, for plurals in D. which are the same in form as the nominative, J. often shows plural by shift of accent, a method which is very common in WJ.; cf. the table given above under the western Juttish definite article; thus, the pl. of *sdi'n* 'stone' is *sdjẽn'*, etc. In EJ. the tendency is to prefer the Juttish ending corresponding to the D. plurals in-er.

To express the possessive case, the ending *-s* is little used except in proper names and with words like *faders- moders-* 'father's, mother's,' etc. On some of the islands in EJ. and from the Mariager Fjord south, the possessive *-s* appears. In WJ., the possessive is almost always expressed by periphrases, as *de ae ae skraedder hans hu's* 'that is the tailor's house'; *de ae de bitte djer daw* 'this is the little ones' day.' 'Whose is this' is expressed: *hvem ae de sin.* In each of the above cases, it will be noted that the periphrasis is made by means of the possessive pronoun: 'this is the tailor his house'; 'this is the children their day'; 'who is it his' = 'whose is it?'

The neuter ending *-t* only appears in some places, notably in SJ., but in most of Jutland we find the neuter of *god* 'good' = *godt* (pr. *gott*), as in SD. The following table will illustrate:

	Adjective		
Danish	EJ.	SJ.	WJ.
et klogt barn 'a wise child'	*klo'q*	*kloxt* *klo'q*[2]	*klo'q*
et stort bord 'a big table'	*sdu'*	*sduet* *sdoe*	*sdu'er*

[1] Cf. Swed. *krig* 'war,' neuter, but in Dan. *Krig* is common gender. Similar gender variations exist between other kindred dialects, as in High Germ. *das Boot* is neuter, but in Plattdeutsch, *die Boot* (fem.); Serb. *prtljag* 'luggage' (masc.), but Croatian *prtljaga* (fem.), etc.

[2] *X* = the guttural *kh*; final *-q* = the hard explosive *k*, as the Arab. *qaf*.

	Predicate Adjective		
Barnet er klogt 'the child is wise'	*klo'q*	*kloxt* *klo'q*	*kloq*
Bordet er stort 'the table is big'	*sdu'*	*sduet* *sdo'e*	*sdu'er*

When the neuter forms differ considerably from the other genders, some special adverbs are made, as *höt* 'highly' from D. *höjt*; *gaat* 'well' from D. *godt*; *dyft* "deeply" from D. *dybt*.

There seems to be almost an infinite number of variations in the adjectival usage. Thus WJ. does not alter the adj. at all for the definite, whereas in other parts of the continent and islands, we find the def. *-e*-ending applied as in SD.; as D. *den gode Mand* 'the good man'—J. *ae go(d) man*, or *ae gode man*.

The pronouns are generally as follows:

SD.	J.
jeg 'I'	*a*; *ae* (southern)
mig 'me'	*mae*
du 'thou'	*do*
sig—'self' (refl.)	*sae*
han 'he'	*han* (Samsö: *haaen*)
ham 'him'	*ham*
hun 'she'	*hu, ho*
hende, hendes 'her'	*hin, hinne* (*hinde*)
den 'it' (common)	*daen*
det 'it' (neuter)	*de, dae*
	et when unstressed, as: *a haa et* 'I have it.'
vi 'we'	*vi*
os 'us'	*wos, os*
I 'ye'	*I*
Eder 'you'	*jer*
de 'they'	*de*
dem 'them'	*dem*
deres 'their'	*djer, dje*

The possessives *min, din, sin*, 'my, thy, his' (reflexive) are in WJ: *mi, di, si*. The use of the reflexive in WJ. has practically disappeared, as instead of *sin, hans* is usually employed, *Per tog hans bog* 'Peter took his (his own) book.' Note the neuter forms *mint, dint, sint* in western and middle Slesvig, but *ent*, for the neuter article *et* does not occur, although it is found in the mediaeval manuscripts. In SJ., however, *sin* = *hans*, as *sin sön bor aassaa der* 'his son alwaso

lives there.' This is clearly due to the influence of the German dialects; *sein, sin.*

The relative is *dae* (SD. *der*), as *jen dae gaaer bajre* 'one that goes better.'

As to the verb, comparatively little need be said, as it is inflected quite along the lines of SD. Thus, the present plural form has largely disappeared, 'I am, thou art, he is,' etc. = *a ae(r), do ae(r), han ae(r), vi ae(r), i ae(r),* de *a(er).* Occasionally with the 2 p. pl., *i* 'ye,' the *-r* of the present is dropped and we find *i ha* 'you have.' This is more peculiar to SJ., where there is also a relic of the earlier *-stu* of the 2 p. sg. *do gikst* 'thou didst go'; *hastu* 'hast thou.' This form also appears in Bornholm.[1]

The weak and strong verbs are formed precisely on the same principle as in SD., except that in J., the *-de* ending of the imperfect appears rarely, often being changed to *-et, -t,* as *vuet = varede* 'lasted, endured'; *svuet* and *suet = svarede* 'answered.' Contraction is common, as *gon = gangen*: *han had ett gon = han var ikke gaaet* 'he had not gone'; note the Juttish use of *have* with the verb of motion, contrary to the practice in SD.

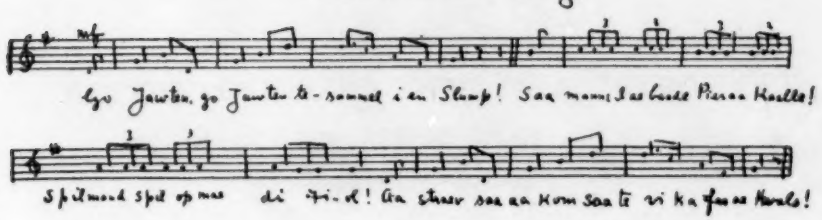
The use of the verbs will be amply illustrated, for the purposes of this sketch, in the preceding selections.

Music

The folkmusic of Jutland does not stand on so high a plane as that of the rest of Denmark. The Juttish melodies are simple peasant songs with not such a musical color as we find, for example, in the music of Bornholm, or even in that of Sjaelland. The following dance song is,[2] however, genuinely Juttish, which is not the case with the songs of SJ., where the German influence is strongly marked.

[1] Cf. *Proc. Amer. Philos. Soc.*, LXIII, p. 207: *taustuin* 'if thou wert to take him.'
[2] *Danmarks Melodibog*, Wilhelm Hansen, Copenhagen; Part I., p. 87.

Juttish Dance Song


Syv Jawtar, syv Jawtar tå-sammel i en Slump! Saa mang Slags lustig Piger Haelle!
Spilmand spil op paa di fri-el! Gaa stivver saa aa kom saa tå vi kan faa en Hvirvle!

Translation

Good evening, good evening, the whole bunch of you!
As many as ye are, both girls and lads!
Musician, play up on your fiddle!
And strive now, and come now,
So we may have a waltz!

THE LEGATION OF THE UNITED STATES OF AMERICA,
BELGRADE.

RECENT DEVELOPMENTS IN SPECTROSCOPY

By R. A. MILLIKAN

(*Read April 29, 1927*)

NEVER in the history of science has a subject sprung so suddenly from a state of complete obscurity and unintelligibility to a condition of full illumination and predictability as has the field of spectroscopy since the year 1913.

It is my privilege to-day to recount the rapidly following steps in this spectacular advance which the Germans call *die Fortschritte der Spectroscopie*,—a peculiarly appropriate phrase since it is so beautifully descriptive of the step by step process by which theory and experiment interplay in rapidly bearing forward the body of our ever advancing knowledge.

I. THE BOHR THEORY

The great step which ushered in the new era was partly theoretical, partly experimental. It was taken when in 1913 Bohr[1] first thought of applying to the interpretation of spectra the then rapidly advancing idea that the frequency of a light-ray might be taken as a measure of the energy lost by its emittor or gained by its absorber—an idea first suggested by Planck[2] in 1900, much more sharply formulated by Einstein, though still without adequate experimental warrant, in 1905,[3] definitely and quantitatively proved for photoelectric effects by a long series of experiments culminating in 1915,[4] and now the firm basis of the whole of modern spectroscopy. It is the most powerful instrument in spectroscopic prediction, and yet an idea which is completely at variance with all nineteenth century modes of thought, and

[1] Niels Bohr, *Phil. Mag.*, 26, 1, 1913; 26, 476, 1913; 26, 857, 1913.
[2] Max Planck, "Wärme strahlung," 1st ed., 1900.
[3] Einstein, *Ann. d. Physik*, 17, 132, 1905; 20, 199, 1906.
[4] *Phys. Rev.*, VII., 362, 1916.

thus far wholly irreconcilable with all the phenomena of interference, which still find their interpretation only in terms of such a spreading wave theory as denies any relation between frequency and energy. This relationship must be regarded simply as a new experimental fact holding in the domain of electronic phenomena, and of as yet unknown relationship to the large scale phenomena for the interpretation of which nineteenth century theories were developed.

And yet this great step of Bohr's, bold as it was, is after all a beautiful illustration of the conservative, step by step, method by which physics has made most of its great advances. Instead of breaking away entirely from the past, as the foregoing comments may have indicated that he did, Bohr actually built conspicuously upon it. He incorporated the whole of celestial mechanics into the structure of his new atomic mechanics, merely adding to the old the features demanded by the newly discovered facts of radiation. Since the positive nucleus of the hydrogen atom attracts the negative electron in accordance with the same law which holds the earth to the sun, there was obviously no procedure consistent with the principle of minimum hypothesis, always followed by science, save to call upon the centrifugal forces engendered by orbital motion to prevent the attracting bodies, whether celestial or electronic, from rushing together under the influence of their mutual attractions.

But these hypothetical electronic orbits confront a condition not met with at all in the case of the equally hypothetical celestial orbits; for a planet or other celestial body has no mechanism for losing any appreciable fraction of its energy in a humanly-speaking finite time, while in glowing hydrogen the atoms are continually interchanging considerable fractions of their energy with ether waves, some times absorbing and some times emitting such waves. The new, experimentally-discovered, law of this interchange, viz. $E_1 - E_2 = h\nu$ in which the two Es represent energy before and after emission, and ν the emitted frequency, h being a constant, simply had to be added to the laws of celestial mechanics to obtain the laws of atomic mechanics.

This addition of the old theory, and the new experimental facts, constituted the essence of the Bohr theory. It was the old theory, *i.e.*, the laws of celestial mechanics, that required that when a hydrogen atom emitted radiation in accordance with the equation $E_1 - E_2 = h\nu$ the electron drop into a new orbit of smaller radius, for in celestial mechanics the size of the orbit is fixed by the energy of the orbital system. The possible radii of the orbits of the electron of the hydrogen atom were thus limited to those whose energies differed by the observed emitted frequencies multiplied by the universal constant h, the dimensions of which were those of *moment of momentum*, and the observed emitted frequencies in hydrogen were such as to require that the possible orbits be limited to those for which the moment of momentum of the electron in its orbit progressed by unit steps, each an integral multiple of h, or, more accurately, of $h/2\pi$.

Bohr thus put into his original picture three elements, the first theoretical, the last two experimental

(1) the laws of celestial mechanics,

(2) the newly observed relation between frequency and energy,

(3) the observed series of frequencies in the hydrogen spectrum.

His boldness lay in two points; first, in that he postulated *non-radiating* electronic orbits and thus denied the universal validity of Maxwell's equations; second, in that he ignored entirely, and purposely, the consideration of a *mechanism* of radiation. How an electron in jumping from one orbit to another of different energy can emit a radiation of frequency proportional to the energy-difference no one has as yet been able to suggest. Bohr simply set down and used the newly-discovered, experimental fact and forgot all about mechanism, a procedure which fifty years earlier had been so effectively used, and so abundantly justified, in the establishment and utilization of the second law of thermodynamics.

The original, simple theory of Bohr quickly had quite as

remarkable quantitative successes as had had two hundred years earlier the corresponding orbital theory, in celestial mechanics, of Copernicus-Galileo-Newton. The latter not only furnished a satisfactory interpretation of the retrograde motions of the planets which it was devised to explain, but it made possible the predictions of the precise instant of eclipses, predictions which observations can now check with extraordinary precision; and therein lies its chief claim to modern acceptance.

Similarly, the Bohr theory not only furnished the first satisfactory interpretation of the hydrogen spectrum which gave it birth, but, because of its orbital assumptions, it enabled the Rydberg spectroscopic constant to be predicted from the accurately known values of the charge of the electron, its mass, and the value of *h*. *Purely spectroscopic observations check this computed value to within one-fourth of one percent*, and therein lay the first brilliant success of Bohr's orbital assumptions, though by no means their only ones.

The second and even more unambiguous success of Bohr's assumptions came three years later and constitutes what I shall call the second forward step in the process of spectroscopic illumination and prediction. It was an experimental step taken when Paschen made his very accurate measurement of the very slight difference in position, or wave-length, of corresponding hydrogen and helium lines. This difference came out precisely as predicted from the four-fold difference in the masses of the central bodies about which the same sort of a body, a single negative electron, was supposed to be rotating in each case.

This altogether extraordinary and accurately quantitative check between prediction and experiment constitutes perhaps as striking credentials for an orbital conception as has ever been met with anywhere in the history of orbital ideas, celestial or electronic. It seems quite uninterpretable save in terms of the conception of two widely different masses both of dimensions minute in comparison with their distance

apart, revolving, just as the sun and the earth on the one hand, the nucleus and its electron on the other, are supposed to do about their common center of gravity.

II. Relativistic Interpretation of Fine Structure

The next important step in the advance of modern spectroscopy was taken when in 1916 Sommerfeld applied Einstein's relativistic considerations to the interpretation of the so-called fine structure of spectral lines, and in so doing brought still further powerful support to the theory of electronic orbits.

Bohr's simple theory had considered only circular orbits, the radius r of a given orbit remaining constant and the azimuth φ alone varying as the electron moved about in its orbit. In its inmost orbit the electron of the hydrogen atom had one unit of moment of momentum, the unit being actually $h/2\pi$, in its second orbit two units, in its third three, etc. But this condition could be as well fulfilled with certain types of elliptical orbits as with circular orbits, but in that case two independent variables φ and r would have to be introduced to describe the motion in each ellipse. Sommerfeld applied the same sort of quantum conditions to both of these variables as Bohr had applied to φ alone before. He made the total moment of momentum[1] of the system—the so-called total quantum number—the sum of an azimuthal and a radial quantum number, the way in which the total moment of momentum[1] was divided between the two determining the ellipticity of the orbit. Thus, when the total quantum number was 1, the azimuthal quantum had to be 1 and the radial quantum number 0, or else the azimuthal 0 and the radial 1. The first condition meant physically a circular orbit, as in the simple Bohr theory, while the last meant a radial oscillation in which the electron had to pass through the nucleus, a physical impossibility. Hence the inmost orbit, designated as a 1.1 orbit (the first integer denoting total, the last azimuthal quantum number, the radial being the difference between the two), had always to

[1] More correctly "quantity of action" defined by $\int p_\varphi d_\varphi + \int p_r d_r$.

be a circle. For total quantum number 2, however, two different orbits were possible, namely, a 2.2 orbit, a circle (later on to be designated a *p* orbit), or a 2.1 orbit, an ellipse (later called an *s* orbit) having its major axis equal to the diameter of the circle and twice its minor axis. The orbits of total quantum number 3 might have the shapes 3.3, 3.2, or 3.1, as shown in the figure (see Fig. 1), namely, a circle and two ellipses all having the same major axis but minor axes in the ratios 3, 2, 1.

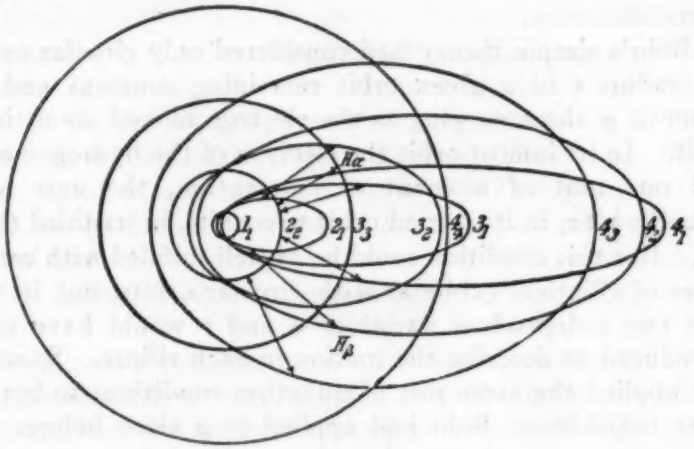


FIG. 1.

So long as the Newtonian law of attraction governed the motions, all the orbits of a given total quantum number would have exactly the same energy (for this would be determined solely by the length of the major axis of the ellipse) and hence be spectroscopically indistinguishable, since the only directly observable quantity in spectroscopy is the emitted frequency, determined by the electron jump from one orbit to another, *i.e.*, by the difference in the *energies* of two orbits. But when the Einstein relativity corrections are applied to the Newtonian laws there is introduced a change of mass with speed which makes the energy of binding of the electron to the nucleus in the case of a series of orbits of the same major axis (total quantum

number) slightly greater the greater the ellipticity of the orbit, and hence makes the electron jumps into these orbits from a given position correspond to a number of slightly different frequencies, thus introducing fine structure into spectral lines. The theoretical procedure here adopted is in part the same as that used by Einstein in explaining the precession in the perihelion of Mercury, *i.e.*, it is another instance of the application of the laws of celestial mechanics to the domain of atomic mechanics. The quantitative prediction, however, of Mercury's precession involves the *general* Relativity theory whereas the foregoing has to do only with "*Special* Relativity."

This Sommerfeld relativistic interpretation of fine structure introduced new possibilities of spectroscopic prediction which from 1919 to 1924 had extraordinary successes. Thus, all the lines of the so-called Balmer series of hydrogen, which consist of jumps into the state of total quantum number 2, a state possessing only two possible orbits, one a circle and one an ellipse, should reveal a fine structure corresponding to the difference in the energies of these particular circular and elliptical electronic orbits. To a first approximation they should actually all be doublet-lines with an accurately predictable difference in wave-length, or in frequency; and again *the prediction could be made wholly from the laws of orbital mechanics*. The theoretical value of the frequency separation of the hypothetical hydrogen doublets came out .365 cm^{-1} frequency units. Now with instruments of high resolving power, not only are these hydrogen lines *all* found actually to be doublets just as predicted, but the most accurate determination of this separation, obtained from $H\beta$, recently made by Dr. William V. Houston in the Norman Bridge Laboratory comes out .36 cm^{-1}—a value within a percent of the computed value—an altogether outstanding and spectacular demonstration of the power of the physicist's newly discovered methods of spectroscopic prediction.

Again, the theoretical relativity formula made the frequency separation of a corresponding pair of lines vary with

the fourth power of the atomic number so that the corresponding lines in ionized helium—like atomic hydrogen a simple nucleus-electron system, but with a charge on the nucleus twice that in hydrogen—should be sixteen times as far separated as in hydrogen. This prediction also was accurately verified, as early as 1916, by Paschen,[1] his observed separation divided by 16 yielding, with extraordinary accuracy, 0.365 ± .0005,—another quantitative triumph of orbital theory.

Equally significant and even more beautiful as a piece of powerful analysis and wonderful prediction was Epstein's work[2] on the effect of a superimposed electrical field in modifying electronic orbits of the foregoing sort, for he thus predicted correctly practically all the intricacies of the Stark effect—a triumph not only of the orbital theory but of the theoretical analysis itself.

But perhaps the most spectacular success of the relativity formula as applied to the interpretation of spectroscopic fine structure came when this formula predicted with approximate correctness the frequency separation of the so-called *L* doublets in the X-ray spectra of the heavy elements like uranium. This, being a doublet corresponding, like the hydrogen doublets, to the electronic orbits of total quantum number 2, ought, if it were actually a relativity doublet, as assumed by Sommerfeld, to have a frequency separation obtainable by multiplying the observed separation in the case of hydrogen by the huge factor of 71 million, for this represents the fourth power of the ratio of the atomic number of uranium 92, to that of hydrogen 1. This prediction also checked with experiment.[3]

No theoretical formula in the history of physics has had more spectacular successes than has Sommerfeld's relativity formula as applied to spectroscopic fine structure.

[1] Paschen. Bohrs, "Heliumlinien," *Ann. der Physik*, 50, 901, 1916.
[2] P. Epstein, "Zur Theorie des Stark Effekt," *Ann. der Physik*, 50, 489, 1916.
[3] See Sommerfeld's Atombau, etc., 4th Ger. Ed., p. 445.

III. Regular and Irregular Doublet Laws in Optics

Another step that introduced new power into spectroscopic prediction was taken about 1920 in the development of high-vacuum "hot-spark" spectrometry,[1] for this enabled Dr. Bowen, Dr. Sawyer, and myself not only to explore new regions of the ultraviolet spectrum and to add largely to our knowledge of spectra, particularly in the case of the light elements, but also to develop new methods for the identification of lines and to discover new relationships that soon forced new interpretations and assisted in the introduction of fundamentally new conceptions into physics. Few more interesting instances of the exact verification of spectroscopic predictions can be found than are recorded in the work on stripped atoms.[2] Indeed the discovery that our high potential vacuum sparks produced such atoms was made by predicting with the aid of the Bohr theory the exact positions and character of the lines to be expected from atoms stripped of all their valence electrons and hence reduced practically to hydrogen-like systems. It was only such reduction that at first made the predictions possible. The predicted lines were all found in the case of all the atoms of the first two rows of the periodic table except fluorine, which holds its electrons too tightly to enable our sparks to strip them all off. There were thus obtained, for the first time in the field of optics, the spectra of a long series of atoms of precisely like electronic structure but varying nuclear charge, namely, the series consisting of stripped Li, Be, B, C, N, O, and the second series consisting of stripped Na, Mg, Al, Si, P, S, Cl.

By the study of these series it was found that two laws found earlier in the field of X-rays and called the regular and irregular doublet laws held also in the field of optics, and they have since constituted powerful instruments for the prediction and identification of spectra. We have found

[1] *Proc. Nat. Acad.*, 7, 289, 1921.

[2] *Proc Nat. Acad., Sci.*, 10, 199, 1924; 11, 119, 1925. *Phys. Rev.*, 25, 295 and 591, 1925; 24, 209, 1924; 28, 923, 1926.

them to be applicable to all series of like electronic structures, whether stripped or only partially stripped. The irregular doublet law enables one, if he can only get started on such a series, to predict the precise place in the spectrum, *i.e.*, the precise wave-length, at which the corresponding lines of the other elements in the series are to be looked for, while the regular doublet law tells him the precise character of the line he should seek, *i.e.*, the frequency separations to be expected in its fine-structure. The foregoing is, then, a step in which the experimental foot goes forward. Next comes again the theoretical step.

IV. Inner Quantum Numbers

The only cause of spectroscopic fine structure thus far considered has been the relativity cause which operates by virtue of a difference in the shapes of orbits of the same total quantum number. But it was found as early as 1920 [1] that that was not enough to account for all the facts of fine structure. In X-rays, for example, the so-called L orbits, or L levels, correspond to a total quantum number 2, and this permits of only two different orbits, one a circle designated as a 2.2 orbit, and one an ellipse designated as a 2.1 orbit; but X-ray absorption experiments brought to light three different levels or orbits, all close together in frequency, in which L electrons were actually found. Two of these followed the relativity law, also known as "the *regular* doublet law" referred to in the last paragraph. The frequency difference of these two levels varied with the fourth power of the atomic number, as demanded by the relativity equation. These two levels are represented by the two diverging lines marked $L_{II}L_{III}$ in Fig. 2. The third level, L_{I}, is seen to follow an entirely different law for it runs everywhere parallel to L_{II}. The frequency difference between L_{I} and L_{II} follows what was called the *irregular* doublet law in the last paragraph, so that the geometrical representation in Fig. 2 of the irregular doublet law is parallel lines, of the regular doublet

[1] Sommerfeld, *Ann. der Physik*, 63, 221, 1920. See also Atombau u. Spectrallinien, chap. 8.

law diverging lines. Similarly, in the field of optics there are found *experimentally* three levels, or orbits, corresponding to the total quantum number 2, instead of merely the two permitted by the relativity theory. Since, then, only two different shapes were possible, Bohr and Sommerfeld introduced the idea that two orbits of the same shape, but of different orientations, might exist, and that some dissymmetry

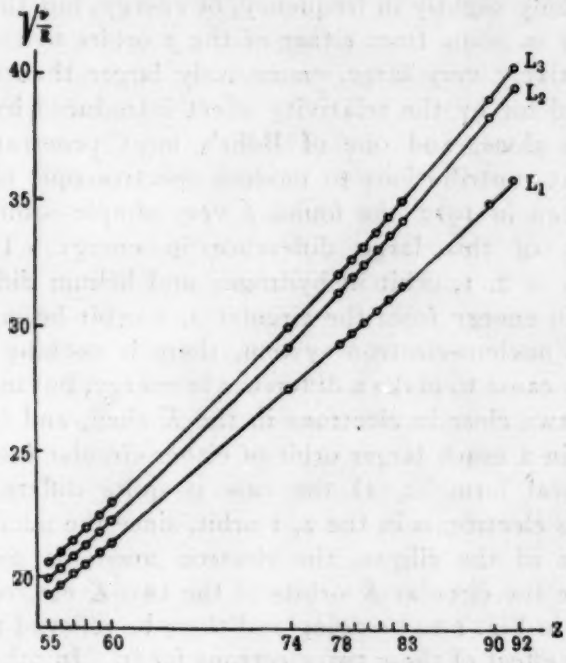


FIG. 2.

in the central force-field gave these two orbits slightly different energies. They introduced a so-called inner quantum number generally denoted by the symbol J to take care of this variation in *orientation*, just as changes in azimuthal quantum number took care of variations in *shape*.

The difference in the frequencies of the familiar doublet lines in lithium, for example, was supposed to be due to the fact that these two lines represented jumps into a common orbit called an *s* orbit from two orbits which differed only

in inner quantum number, *i.e.*, orbits of different orientations but the same shapes,—in this case circles, or 2.2 orbits, known as the p_1p_2 orbits. The *s* orbit, on the other hand, into which these two electrons jumped to form the lithium doublet, was the third possible orbit of the total quantum state 2, namely, the 2.1 orbit, of elliptical shape, the major axis being twice the minor. The two circular, or *p*, orbits differed only slightly in frequency, or energy, but the change in energy in going from either of the *p* orbits to the *s* orbit was relatively very large, enormously larger than could be accounted for by the relativity effect introduced by change in shape alone, and one of Bohr's most penetrating and important contributions to modern spectroscopic ideas was made when in 1922 [1] he found a very simple orbital interpretation of this large difference in energy. Thus the elliptical, or 2, 1, orbit in hydrogen and helium differs only slightly in energy from the circular 2, 2 orbit because, with a simple nucleus-electron system, there is nothing but the relativity cause to make a difference in energy, but in lithium, with its two close in electrons in the *K* shell, and its one *L* electron in a much larger orbit of either circular form (2, 2) or elliptical form (2, 1) the case is quite different. For when this electron is in the 2, 1 orbit, since the nucleus is at the focus of the ellipse, the electron must, at perihelion, dip inside the circular *K* orbits of the two *K* electrons as it is shown in Fig. 1 to be doing and there be relieved from the screening effect of these two electrons for it. In other words because of this *penetration of the elliptical orbit inside the K shell* the electron when in the 2, 1 orbit gets into a field of force several times as intense as it would if it did not penetrate the *K* shell. It is thus, *on the average*, very much more tightly bound to the nucleus than is the electron in the 2, 2 orbit which does not thus dip inside the *K* shell. Hence wherever there is such an inner shell to penetrate the energy difference between the 2, 1 and the 2, 2 orbits will be very much larger than the relativity difference. The very beauti-

[1] N. Bohr, "Three Lectures on Atomic Physics" Vieweg Braunschweig, 1922. Also *Ann. der Physik*, 71, 228, 1923.

ful way in which this sort of explanation fits the observed facts whenever there are inner shells to penetrate constitutes another extraordinary triumph for the orbital conception.

This Bohr mode of interpretation thus called on differences in *shape* with the consequent differences in interpenetration to account for the difference in energy between the p and s orbits, and left only differences in *orientation* with the same shapes, to account for the difference in energy between the p_1 and the p_2 orbits.

V. The Spinning Electron

Now the extraordinary and disconcerting fact which Dr. Bowen and I brought sharply to light as soon as we had obtained and hence could compare the spectra of a long series of stripped atoms like Li, Be, Bo, C, N, O, all of which had exactly the same electronic structure but varying nuclear charge, was that *the difference in energy between the two circular, p_1p_2, orbits varied with atomic number in precisely the way demanded by the relativity equation.*

There had been indications of this relation earlier. Sommerfeld in the first edition of his book, 1919, had suggested it, but in later editions had discarded it as untenable because theoretically impossible and not experimentally justified. No unambiguous evidence could in fact be got until we had obtained a long series of atoms of identical electronic structures and varying nuclear charge to compare. We then found to our amazement, by careful quantitative studies, that *the energy difference between the p_1p_2 orbits followed in every respect the relativity equation*, though it could not possibly be due to a relativity cause, since this cause required differences in *shapes* of orbits, while the p_1p_2 orbits could not have differences in shapes but only differences in orientation.

In papers published in 1924 and 1925[1] we therefore stated it as one of the most interesting problems of theoretical physics to retain relativity as a cause of fine structure in hydrogen and helium, and indeed in general, and yet to find

[1] *Physical Review*, 24, 209–228, 1924. *Proc. Nat. Acad. Sci.*, 11, 119, 1925. *Phil. Mag.*, 49, 923, 1925.

another non-relativistic cause "magnetic, or magnetic and electrostatic combined," which would follow exactly the relativity equation.

Probably never before in the history of physics had such an extraordinary—so well nigh impossible—a condition been imposed. And yet within a year of our statement of the problem, two young Dutch physicists, Uhlenbeck and Goudsmit [1] stimulated partly by our work, partly by other difficulties with existing theory, pointed out especially by Landé, and having to do with the so-called anomolous Zeeman effect, had found *in the assumption of the spinning electron* another cause of fine structure which followed exactly the same law in all respects as the relativity cause. The incident furnishes as striking an illustration as the history of physics thus far affords of the power of experimental and theoretical methods combined for predicting new phenomena, interpreting old ones, and thus slowly but inevitably, step by step, forcing open nature's hitherto fast bolted doors.

The new physical conception introduced by Uhlenbeck and Goudsmit is that every electron within an atom is not merely revolving in an orbit, but at the same time rotating, just as does a planet, on its own axis. There are assumed to be but two possible directions of spin, 180° apart, but the moment of momentum of spin is assumed to be always the same, namely, exactly one half unit of moment of momentum, *i.e.*, $\frac{1}{2}\ h/2\pi$. Such a conception introduces precisely the right amount of energy-difference between the p_1p_2 circular orbits which is necessary to account for their observed spectroscopic frequency separation. This effect is simply superposed upon the relativity effect, thus making the fine structure, even in hydrogen and ionized helium, somewhat more complex than could be accounted for by the relativity effect alone. *This newly predicted complexity has just been found by new and more refined measurements by Dr. Houston to fit the experimental facts much better than did the old theory.*

[1] Uhlenbeck and Goudsmit, *Nature*, 117, 264, 1926.

VI. The New Spectroscopic Rules

With the aid of this new conception it has now become possible to make a nearer approach than heretofore to presenting a physical interpretation of a group of remarkable spectroscopic rules developed, largely empirically, within the past two years by Russell,[1] Heisenberg,[2] Pauli[3] and Hund,[4] and embracing in an altogether remarkable way, most of the facts of spectroscopy known up to the present. The success with which these empirical rules describe the facts of spectroscopy is little less than magical. These rules naturally all start with, and grow out of, the fundamental assumption underlying all quantum theory, namely, that all periodic motions must be quantized, *i.e.*, that periodic motion itself is unitary in its nature. As applied to atomic mechanics this means simply that all moments of momentum characteristic of the periodic motions within the atom are to be assigned characteristic quantum numbers and are to be allowed to change only by unit steps.

In the case of each individual electron there are just four sorts of such moments of momentum to consider. In other words there are four elements necessary to the complete description of an electron's motion within the atom, namely (1) the size of its orbit, (2) the shape of its orbit, (3) the orientation of its orbit in space and (4) the orientation, or direction, of its spin.

(1) The total moment of momentum (quantity of action, see p. 215) of an electronic orbit is characterized by its total quantum number n introduced by Bohr. This fixes the size (or major axis) of the orbit.

(2) The azimuthal quantum number which, with a given n or major axis, fixes the shape (minor axis) of the orbit has heretofore been characterized by the quantum number k. For some reason not yet fully understood, but doubtless of

[1] Russell and Saunders, *Astrophys. Jr.*, 61, 38, 1925.
[2] Heisenberg, *Zeit. f. Phys.*, 32, 841, 1925.
[3] Pauli, *Zeit. f. Phys.*, 31, 765, 1925.
[4] Hund, *Zeit. f. Phys.*, 33, 345, 1925.

profound physical significance (see below) in order to make the new spectroscopic rules fit the experimental facts it is found necessary to reduce by unity all values of k heretofore assigned. Since, however, we are not yet ready to discard entirely for the old purposes the old interpretations, this reduced value of k is for convenience denoted by a new symbol l, so that merely by definition $l = k - 1$. Thus for an s orbit $l = 0$, for a p orbit $l = 1$, for a d orbit $l = 2$, etc.

(3) The projection of the orbital moment of momentum l upon any fixed direction of reference, which, in the consideration of the Zeeman effect, is the direction of the applied external magnetic field, is quantized and designated by the symbol m_l. This projection obviously fixes the orientation of the orbit in space. The physical significance of the fact that this projection is quantized is that only certain definite orientations of this orbit are possible (such spacial quantization is directly proved by the so-called Stern and Gerlach experiments.)

(4) The projection of the moment of momentum of spin upon the fixed direction of reference is designated by the symbol m_r. As stated above, there are supposed to be in each atom but two possible directions of spin, 180° apart; so that m_r of course determines in which of these two directions a given electron is spinning. The quantities m_l and m_r are usually called magnetic quantum numbers merely because of their use in connection with magnetic fields.

Now one of the new and very illuminating spectroscopic rules known as the Pauli exclusion rule states that in a given atom no two electrons may be alike in all four of the above elements; in other words two electrons cannot occupy one and the same electronic position within an atom.

This rule carries with it at once a whole group of conclusions which have been reached by piecing together evidence from many quarters. Thus it requires that the K shell of all atoms possess two electrons and no more; for since in this shell $n = 1$ and $l = 0$ and hence $m_l = 0$ it follows from the rule that in the fourth element of their motion, namely

the spin, the electrons must be different, and also that not more than two can exist without having two alike in all four elements, and hence violating Pauli's Rule.

Similarly, and for precisely the same reasons, there can be but two electrons in *s* orbits in a shell of any total quantum number whatever, and this in turn requires that the eight electrons of the *L* shell shall be found, two in *s* orbits and six in *p* orbits—a relation discovered by Stoner [1] and Main-Smith [2] in England in 1924 by putting together evidence from a variety of sources.

Further, because of the opposite directions of spin of the two electrons of a *K* shell their joint or resultant moment of momentum and also their total magnetic moment must be zero, which in turn requires that helium be diamagnetic as it is found in common with all the noble gases actually to be.

Pauli's Rule also requires that every closed shell, indeed every completely symmetrical electronic configuration, have a zero value of its resultant moment of momentum, and also of its magnetic moment. This accords with the fact that mercury, and other two valence atoms, in their ground, or unexcited, states are diamagnetic.

Since every completed shell has zero moment of momentum, it follows at once that the total or resultant moment of momentum of an atom having a given electronic configuration [3]—and the number of different values that this moment can assume determines of course the number of terms in the fine structure corresponding to that configuration—must be made up of the combined moments of momentum of the electrons in the uncompleted or valence shell. This important relation was first perceived by Russell [4] who also

[1] Stoner, *Phil. Mag.*, 48, 719, 1924.

[2] Main-Smith, *Jour. Soc. Chem. Ind.*, 44, 944, 1925.

[3] The word configuration as above used means the definite distribution or assignment of the valence electron to their proper types of orbit, *e.g.*, s^2p^3 means two electrons in *s* orbits and three in *p* orbits. Again $s^2p^3 \cdot s$ means that one of the three *p* electrons, just referred to, has been pushed up into a higher state. Since there are a total of 6 *p* orbits, 3 electrons can be fitted into the 6 orbits in a considerable number of different ways and to each way corresponds a characteristic term value.

[4] Russell and Saunders, *Astrophys. Jr.*, 61, 38, 1925.

first formulated the new rules for the composition of the total moment of momentum of the atom from the foregoing components. Thus when more than one electron is present in the valence shell the joint orbital moment of momentum L of the whole group is obtained by taking the quantized vector sum of the individual moments l. For example, for two electrons in p orbits, for each of which $l = 1$ the quantized vector sum is 0, 1 or 2. The physical significance of the fact that the vector sum of the ls is quantized to obtain L is that the electrons are able to rotate only in orbits of such orientations about the nucleus that this vector sum is a whole number of units of moment of momentum.

The next step is to obtain the joint moment of momentum R of the spins of the individual electrons. Since these spins are assumed to be in the same plane and each of amount $r = \frac{1}{2}$, the quantized vector sum is here simply the algebraic sum, *i.e.*, in this case $R = \frac{1}{2} - \frac{1}{2} = 0$ or $\frac{1}{2} + \frac{1}{2} = 1$.

Finally to obtain the total moment of momentum of the whole atom we take the quantized vector sum of the total orbital moment L and the total spin moment R. This is precisely the quantity which was originally called by Sommerfeld the inner quantum number and designated by the letter J. Thus for the values $L = 2$, $R = 1$ the quantized vector sum is 1, 2 or 3.

The fundamental quantum condition is now that all possible values of J constitute a series of which the successive steps differ by unity. If the value of R is an odd number of half units, *i.e.*, if there are an odd number of spinning electrons then all the values of J are obviously half integral ($\frac{1}{2}$, $\frac{3}{2}$, $\frac{5}{2}$, etc.) while if the value of R is integral, then all the values of J are likewise integral. The number of possible values of J obtained from a given pair of values of R and L gives the multiplicity, *i.e.*, the number of terms in the fine structure. The maximum of the multiplicity is actually $2R + 1$.

The notation now in general use in the formulation of the new rules is as follows. When the value of L built up as above from the vectorial summation of the ls of the individual

orbits is 0, the term is by definition an S term. When $L = 1$ the term is by definition a P term; when $L = 2$ it is a D term; when $L = 3$ it is an F term, etc.

The foregoing quantization rules predict a much larger number of terms than are actually obtained from a given configuration, but Pauli's exclusion rule succeeds in reducing the number of terms to those actually observed.

The new rules thus briefly outlined have had such altogether extraordinary success in predicting the character of the spectra emitted, not only by the simpler atoms, such as those which have been the subject of the studies of Dr. Bowen and myself, but by atoms like iron and titanium, thousands of whose lines have already been identified, that it seems as though we now have within our grasp the means of predicting the precise sorts of radiations which can be emitted by all the possible excited states into which any atom can be thrown. Indeed the predictions even of complicated spectra have recently been so successfully made that one prominent spectroscopist has lately remarked that "the heroic age of spectroscopy is already past."

Nevertheless, these rules are still to a considerable extent empirical—a remarkable witness to the ingenuity of the physicist in picking out *rules of behavior*, and extrapolating from these rules from observed to unobserved phenomena. They are however little more than *rules*. They do not yet represent a logical and consistent scheme of interpretation, and they are only vaguely and very imperfectly translatable into physical pictures visualizable in terms of physical relationships. Thus, after the extraordinary and manifold successes detailed above which followed upon Sommerfeld's introduction of the idea of elliptical orbits—an idea definitely requiring that one unit of azimuthal moment of momentum be assigned to s orbits, two to p orbits, etc.—it is disconcerting in the extreme to find that to fit the new spectroscopic rules s electrons can have no orbital momentum at all ($l = 0$). This looks like a very fundamental contradiction and seems to spoil a whole group of interpretations which were thought

to be quite definitely established. It is, however, just such contradictions which point the way to the next advance, as was so beautifully shown in the foregoing history of the development of the idea of the spinning electron. Furthermore this advance is perhaps already dimly in sight, for it has just been pointed out by Epstein that the new Schroedinger mechanics actually does require that the *s* state (orbit) always represent a pure radial pulsation, centrally symmetric, and without any axial structure, and hence without any angular momentum. The whole moment of momentum of electrons in the *s* state would then inhere in the spin. But just what influence this new group of ideas is to have upon the whole group of orbital conceptions developed in the early portion of this review is still veiled in obscurity.

The last fourteen years of the history of spectroscopy, however, constitutes a remarkable illustration, first, of the rapidity of our modern rate of advance—a whole huge domain, a veritable dark continent—having been explored and reduced to order and civilization in a period of less than fifteen years; and, second, of the power of the physicist's two tools, analysis and experiment, when used properly together, for forcing open nature's most tightly barred and bolted doors and wresting her most jealously guarded secrets from her for the enrichment of the life of future generations. For every bit of added knowledge of nature adds so much to our control over her, that is, to our ability to turn her hidden forces to useful ends.

ACCOMPLISHMENTS OF MODERN ASTRONOMY

By C. G. ABBOT

(*Read April 29, 1927*)

It is natural to ask such questions as the following:

1. How many stars are there and how are they arranged?
2. How far away are the stars and how large?
3. What do we know of the constitution, age, and source of energy of heavenly bodies, and what relations hold regarding their temperatures and rays they constantly emit?
4. Whither and how fast are the heavenly bodies moving?
5. What is the bearing of astronomical discovery on the province of life?

There are many other interesting astronomical questions, but a vivid impression of the greatness of the accomplishments of recent years will come from a survey of these five alone.

1. It was long ago suggested that if the stars are infinite in number, and if space is infinite in extension, the whole vault of the heavens should glow as brightly as the sun. The interposition of absorbing matter would not alter this conclusion, because, according to Kirchhoff's law, the whole interior of a space with walls of constant temperature takes up the temperature of the walls. Hence absorbing matter surrounded by an infinite army of stars would at length be as hot as they.

Obviously, therefore, the stars are not infinitely numerous and they occupy little volume relative to the space in which they lie. In order to estimate the numbers of stars, a careful study of the numbers brighter than certain magnitudes has been made. First of all, however, the scale of brightness required to be standardized. Very anciently, some of the brighter stars were called of first magnitude, others less bright,

such as the Pole star, of second magnitude, and so on. It was found that the values assigned by these ancient estimates could be nearly duplicated by assuming that an increase of five magnitudes produces exactly 100-fold decrease in apparent brightness, and that the logarithm of the change of brightness for one magnitude is

$$\frac{\log. 100}{5} = 0.4000.$$

With this exact definition of what had formerly been a rough and ready classification, several observers have attained great distinction by their long-continued researches in stellar photometry, which means the assignment of exact magnitudes to the stars. Some of these investigators made visual observations at the telescope, but, as in other lines in astronomy, photography was employed by others with great advantage. The ordinary photographic plate is most sensitive to blue and violet light, while the eye is most sensitive to light-green and yellow. Hence, while some observers were contented to measure photographic brightness by ordinary plates, others interposed absorbing screens suitable to restrict the rays to the most effective visible region, and observed on orthochromatic plates.

Thus the values of brightness of the stars have been measured not only photographically, but photovisually, as we may say, and the magnitudes have been recorded on both scales. Reddish stars are naturally much fainter according to photographic than according to visual or photovisual magnitudes. The difference of the two scales gives, indeed, a measure of the effective colors of stars, which, in cases of objects too faint for spectroscopic analysis, is valuable as an index of the character of the spectrum.

By persevering work along these lines, there have been established in recent times standard series of stars of fainter and fainter apparent luminosity, so that the observer in any part of the heavens, by making comparison-photographs from time to time within standardized stellar regions, may reduce

his observed magnitudes to a well-settled standard scale, no matter what the prevailing sky conditions for the night may be.

With these advantages, it has become possible to count accurately the numbers of stars brighter than specified magnitudes, as photographed within selected areas, each of small but definite extent, and numerous enough to give a fair estimate of the star population of the whole heavens. In the following table, prepared by Seares and van Rhijn, we see how the numbers run.

TABLE I

NUMBERS OF STARS BRIGHTER THAN A GIVEN MAGNITUDE

MAG. LIMIT	NOS. OF STARS		RATIO VISUAL	MAG. LIMIT	NOS. OF STARS		RATIO VISUAL
	Photographic	Visual			Photographic	Visual	
4.....	360	530	3.1	12	1,100,000	2,270,000	2.6
5.....	1,030	1,620	3.0	13	2,720,000	5,700,000	2.5
6.....	2,940	4,850	3.0	14	6,500,000	13,800,000	2.4
7.....	8,200	14,300	2.9	15	15,000,000	32,000,000	2.3
8.....	22,800	41,000	2.9	16	33,000,000	71,000,000	2.2
9.....	62,000	117,000	2.8	17	70,000,000	150,000,000	2.1
10.....	166,000	324,000	2.7	18	143,000,000	296,000,000	2.0
11.....	431,000	870,000		19	275,000,000	560,000,000	1.9

Thus the total numbers of stars brighter than a given magnitude increase nearly by ratios of 3.0-fold as we pass from step to step, until we come down to stars of magnitude 9, which are some 50 times fainter than the naked eye can see. Then the ratio of increase grows steadily smaller until at the 19th magnitude the ratio has diminished to 1.9. The relation of this ratio of increase to the magnitude number may be expressed by a suitable formula.

With this mathematical expression it is easily possible to proceed beyond the 20th magnitude to integrate the probable number of stars, on the assumption that this formula, which holds so well down to the faintest telescopic magnitudes, still continues to retain its validity beyond. In this way Seares has computed that our galaxy of stars probably contains

about 30,000,000,000 stars, or some 20 times as many stars as there are living human inhabitants of the earth.

Looking in the direction of the constellations of Cassiopeia, Aquila, Sagittarius and Crux, we see the hazy brightness of the Milky Way, which, when examined with a telescope, resolves itself into a multitude of stars. The Milky Way is easily seen to be a nearly flat ring, extending completely around the heavens, and is frequently taken as the datum plane from which to chart star positions. Thus astronomers speak of Galactic latitude and longitude.

If the average numbers of stars per square degree brighter than a given magnitude are classified with reference to their Galactic latitudes, it is immediately seen that towards the Galactic poles they are few compared to the numbers near or in the Galaxy itself. In the following table (after Seares and van Rhijn) this information is collected:

TABLE II

GALACTIC CONCENTRATION OF THE STARS

Numbers of Stars per Square Degree Brighter than a Given Photographic Magnitude

LIMITING MAGNITUDE	GALACTIC LATITUDE			
	0° Number	20° Number	40° Number	90° Number
7.....	0.36	0.22	0.14	0.10
11.....	20.8	11.6	7.3	4.3
15.....	910.	400.	199.	87.
19.....	20,750.	6,860.	2,180.	770.

How shall we explain this inequality of distribution of the stars and more especially of faint ones? Either their ranks extend much farther along the plane of the Milky Way than towards its poles, or else a pair of veils of progressively increasing obscurity, like two caps, one on either side of the Galaxy, hide from us the multitude of stars which otherwise might be seen towards the Galactic poles. The second hypothesis is too fantastic. Astronomers agree, therefore, that our system of some 30,000,000,000 stars is contained in a lens-

shaped space some five times as extended in diameter along the Galactic plane as at right angles thereto.

2. When Copernicus conceived the sun to be the center of the earth's orbit, and the apparent motions of the stars to be merely the consequences of the earth's daily axial rotation, he required of his disciples for the next three centuries a great faith. For they must believe that the stars are all so distant that the immense circuit of the earth about the sun neither seemed to bring any stars nearer, nor even caused them to appear to approach and recede, one from another, as trees do when we pass a forest. It was not until about the year 1840 that the most refined measurements disclosed any angular displacements of stars attending the annual revolution of the earth. Then three star distances were measured by Bessel, Henderson and Struve, respectively, and found to be between twenty and seventy trillion miles.

At the beginning of the twentieth century less than a hundred star distances were known. About that time a new method was introduced, in which photography was substituted for eye observation at the telescope. By dimming a bright star in the center of the telescopic field, so as to compare equally with very faint stars about it, we may obtain at six-month intervals properly exposed photographs. Since the very faint stars will almost always be faint because of immense distances, the photographic image of the artificially dimmed star, probably much nearer, will often be found to wander very slightly to and fro among neighboring star images in the six-month intervals, owing to the earth's annual revolution. By assiduous and careful investigations of this kind, which have been carried on at several observatories, these parallactic angular displacements have been determined for nearly 2,000 stars in the past 25 years. Even with the utmost accuracy, however, it is impossible to do more than show that a star is relatively a distant one, if it lies beyond the distance requiring of light 500 years to traverse.

As comparatively few of the stars lie within 500 light years' distance from the earth, we should be little satisfied

unless there were other methods of extending our knowledge of star distances. As concerning measurements of the distances of individual stars, there are two principal methods adapted to stretch the limit of measurement considerably further. The first depends on the existence of numerous pairs of stars in mutual revolution about their common centers of gravity.

Although too near together to be distinguished as double by the telescope, the duplicity of stars is sometimes recognized by the regularly recurring variation of their light, owing to their mutually eclipsing one another. In many other cases the spectroscope detects duplicity by the periodic shifting of the spectral lines, as the stars tend alternately to approach and recede from the earth in their orbits. However, it is not of such close double stars as the eclipsing and the spectroscopic binaries that distances are determined, but of the numerous pairs of stars seen telescopically separate, and observed sufficiently long to trace a considerable part of their apparent orbits in the heavens.

In such cases the law of Kepler informs us:

$$M_1 + M_2 = A^3/P^2,$$

where M_1 and M_2 are the masses of the two stars compared to the earth plus the sun, A, the semi-axis of their orbit in terms of the semi-axis of the earth's orbit, and P the period of rotation measured in years. If $M_1 + M_2$ were known, A could be computed. Fortunately the study of eclipsing and spectroscopic binaries has proved that for most double stars the combined masses differ little from double the mass of the earth and sun. Hence it is assumed in this method of obtaining star distances that $M_1 + M_2 = 2$.

If so

$$A = \sqrt[3]{2P^2}.$$

Inasmuch as the factor of the combined mass enters in the cube root, the results would be only 100 per cent in error if $M_1 + M_2$ were really 16 instead of 2, so that measurements of

A, made in this manner, are generally of comparatively high accuracy. Since the angular measure of *A* is readily observed with the telescope, the star's distance from the earth corresponding to the linear measure found by the application of Kepler's law follows immediately. Nearly 1,000 stellar distances have been determined by this, the so-called "dynamical" method.

A still more fruitful means of measuring star distances depends on a close scrutiny of certain absorption lines in their photographic spectra. In the latter years of the nineteenth century the spectra of the stars were arranged at the Harvard College Observatory in certain great classes called B, A, F, G, K, M, ranging from the blue stars of Type B, through the solar stars of Type G, to the red, or Antarian stars of Type M. Certain gradations of these great classes are recognized, such as B5, meaning a type half way between the most extreme, or Bo stars, and the most extreme varieties of Type A, called Ao.

It is found, however, that two stars both of the type G5, for instance, though generally identical in spectrum, may show a few of their characteristic spectral lines in very different intensities. These minute spectral differences have been shown, from both observational and theoretical standpoints, to be due to differences in size of the stars. If two such stars were at equal distances, so as to display their absolute magnitudes, or true luminosities, the one of larger diameter would, of course, be the brighter. The distances of a great many stars have been so well determined by the trigonometric and dynamical methods that it has become possible to fix a scale of appearance of these telltale spectral lines, such that, knowing their appearance in a given star, its absolute brightness is immediately assigned. Knowing, also, its apparent brightness from photometric observations, the distance follows at once. In this way the distances of nearly 3,000 individual stars have been measured spectroscopically.

Omitting in this hasty sketch distances determined by several special methods of limited application, we come finally to the wholesale methods depending on apparent

brightness and on apparent angular motion. It is sufficiently obvious that if all stars were of identical spectrum and identical luminosity, their brightness would vary as the square of their distance. By making allowance for spectrum, and assuming that large and small stars are mixed in the same proportions in the distant parts of the universe that they are in those relatively near, formulæ have been devised which give the average relative distances of stars of the successive stellar magnitudes. It is also obvious that on the average of great numbers the apparent angular motion of stars, or so-called "proper motion," must be inversely as their distances. This wholesale method of estimating stellar distances in terms of proper motions has been extensively applied, and appropriate formulæ have been devised to express it.

These wholesale formulæ are checked by the known individual distances of several thousand stars. They enable us to treat statistically such majestic problems as the absolute size of our stellar system, which we call our Galaxy. It is estimated to be approximately 100,000 light years in its maximum and 20,000 light years in its minimum diameter. Its 20 nearest stars are from 4 to 15 light years distant from us.

Knowing the distances of several thousand stars individually, it has been found that our sun is near the middle rank in absolute brightness, although, to us, it seems brighter than Sirius. Yet, in fact, Rigel is about 10,000 times brighter than the sun, and the sun about 10,000 times brighter than the faintest stars yet known.

As for the sun, its diameter has long been known to be 860,000 miles. Only recently have the diameters of some other stars been determined. This has been done in some cases by direct measurements with the interferometer according to the method of Michelson, but only for a few stars, including Betelgeuse, Antares, Arcturus, Aldebaran and some others. The gigantic red stars first named are found to be from 200,000,000 to 400,000,000 miles in diameter, or several hundred times that of the sun. On the other extreme, several bright stars, including Sirius, are found less than twice the

diameter of the sun. If it were possible to carry the measurements to very faint stars, doubtless some would be found much smaller than the sun. Several indirect methods, depending on the distribution and intensity of light in the spectra of the stars, enable us to estimate the diameters of some of the very faint stars, and this confirms our expectation of their relatively minor size. One yellowish white star, indeed, appears to be no larger than our earth.

3. Since there have been found no stars of masses many times greater than that of the sun, but a whole class of gigantic red stars several hundred times the sun's diameter, and therefore millions of times the sun's volume, it follows that the density of these red giants is of an order a thousand times less than atmospheric air. The study of eclipsing variables, many of which are blue and white stars, has also indicated a range of densities, not, indeed, so excessively rare, but still of the order a thousand times less than the sun's.

As there are many stars of the greatest similarity in size and spectrum to the sun, we must believe that these approximate the solar density, which is 1.4 times that of water. Yet this is by no means the maximum limit of density, for the companion to the brilliant star Sirius has lately been proved to be some 60,000 times as dense as water, or 3,000 times as dense as platinum! To add to the astonishment which this statement evokes, this star, like all others, is wholly gaseous, and is even in that state of great freedom of internal motion called perfectly gaseous.

At ordinary temperatures, gases may be compressed in the laboratory far beyond the density of water, but their molecules then occupy such large volumes compared to the spaces between, that the internal motions of highly compressed gases are hindered, and they are no longer perfect gases. But at the immense temperatures which prevail within the stars, not only are the molecules disassociated, but their atoms also are ionized, so that in place of the relatively bulky molecules, the separated electrons and nuclei only remain. These primitive particles are so excessively small that stars may be compressed

far beyond the densities of our heaviest solid substances, without losing their perfectly gaseous qualities.

Thus the interiors of the stars are in the simplest possible condition for theoretical studies. Eddington and others have taken full advantage of this favorable condition, and have worked out so thoroughly the relation of pressure, temperature, diameter, energy and constitution, and with such excellent accordance with what can be astronomically observed among the host of heavenly bodies, that we may be said to know far more of the internal condition of the stars than we do of the interior of the earth.

It appears that starting with the red giants at less than 3,000° Absolute C. of surface temperature, the successive classes K, G, F, A and B exhibit a scale of constantly rising surface temperatures, culminating with approximately 16,000° Absolute C. for the surface of such a star as Rigel. Our own sun, at 6000° Absolute C. is medium in this as in other characters.

Within the stars two forces strive against the enormous compressive force of gravitation. These are, first, the agitation of the electrons and atomic nuclei, after the fashion of pressures in all gases, and, second, the pressure of radiation. In the interior of stars, the temperatures, which may reach 40,000,000°, are so exalted that the radiation therein exerts perfectly tremendous pressure. At such temperatures the prevailing wave-lengths are of the order of X-rays, rather than of visible light. Such very short wave-rays are unable to penetrate more than a few centimeters through the ionized atomic gas of the stellar substance. Under these circumstances the energy of the interior is imprisoned, and after reaching the blue stage, the central parts of a star grow hotter and hotter, though they are quite unable to prevent the exterior from cooling, back through the successive spectrum classes A, F, G, K and M, with constantly increasing density. Hence we find two kinds of red and yellow stars, one sort very bulky and rare, the other relatively very small and dense. Our sun belongs to the dwarf class, having passed the acme of brightness.

Until recent years the source of energy which maintains the tremendous outward flow of stellar radiation was unexplained. Now, however, the atoms are shown to consist entirely of groups of equal electric charges, positive and negative, supposed to remain separate by virtue of orbital motion. It is conceived that under the circumstances of exalted temperature and pressure prevailing within stars, union of these electric particles may occur, with annihilation of mass, and liberation of energy. If so, a source adequate to supply stellar energy for immense periods of time is available.

From the known mass of the sun it is computed that if it were completely annihilated with evolution of energy, the supply would suffice to maintain the present rate of solar radiation about fifteen trillion years. So the present view ascribes enormous ages to the stars, abundantly sufficient to satisfy the most extravagant time demands which biologists and geologists may be led to make from their studies of existing life and the palæontological records of the past.

The surface temperatures of the stars are estimated by measuring the distribution of the energy of their radiation in the spectrum. As the blacksmith's iron glows with a different color according to the degree of its heat, so do the stars. Laboratory experiments have developed laws and formulæ, connecting the temperatures and energy spectra of sources, which, like the stars, emit continuous spectra. It is by applying these relations that star temperatures are determined.

The surface temperatures of the hotter stars exceed any which we are able to maintain continuously in the laboratory. Hence it is very useful that Anderson has recently invented a method of exploding wires by tremendous electric discharges, able to yield instantaneous temperatures of 20,000° to 30,000°. This permits the experimental study of radiation from such hot sources.

It was a mystery until recently why the profusion of intense Fraunhofer lines which exist in the spectra of yellow stars, like our sun, should yield to fainter and fewer lines for still hotter stars. Indeed, in the blue stars of Type Bo the

visible and photographic spectra are almost devoid of lines. We now understand that the absence of lines does not mean the absence of the chemical elements which produce them. Rather the elementary atoms, under the tremendous excitations prevailing in these very hot star surfaces, have lost several of the electrons of their outer orbits. They are "stripped," as physicists say, and the residual fragments of such stripped atoms give spectra whose lines are beyond the short-wave limit of spectra transmissible by our atmosphere.

If it were not for a high level layer of atmospheric ozone, so scanty that if brought to the earth's surface it would make a gaseous mantle no thicker than a sheet of cardboard, the sun and stars would send us rays of much shorter wave-lengths. In these extreme ultra-violet regions would be found the rich line-spectra of the stripped atoms of the hotter stars.

All that we know of astronomy depends on radiation. But the nature of radiation itself is more mysterious than was supposed 30 years ago. At the end of the nineteenth century it was agreed that radiation is a transverse wave-vibration, set up in the luminiferous ether by the stimulus of the violent internal motions of the molecules of heated substances. All of the phenomena of the propagation of light, highly complex and varied though they are, seemed to be explained satisfactorily in this way. Yet now the newly gained knowledge of the composition and interior structure of the atoms, including the phenomena of radio-activity, seems to require a reconsideration of the older hypothesis of corpuscular radiation, which Newton preferred. In short the emission and the absorption of rays seem to demand a discontinuous corpuscular explanation, while the propagation of rays no less imperiously demands the acceptance of the vehicle of continuous wave-motion. These antagonistic views now await reconciliation.

4. The observations of such men as Bradley, and all of his successors who have measured accurately the positions of the stars, form a precious heritage. They have rendered their makers' names immortal. For it is by comparing these early star-positions with the corresponding ones of to-day, that a

knowledge of the angular displacements, called proper motions of the stars, is determined. All the stars, including our sun, are on the move. The most rapid star moves over 10 seconds of arc per year. Now that stellar distances, as well as proper motions, have become known, these angular displacements are easily converted into linear velocities, though of course seen in projection on the celestial sphere.

Within the past 30 years another element of stellar motion has been added, for the spectroscope has determined for us the rates of approach or recession of several thousand stars. This, too, is but partial knowledge, for it gives only the component of a star's true motion, relative to the earth, which is projected upon a radius of the celestial sphere. Yet when the proper motions and the radial motions of the stars are combined, as they can now be, we obtain the true motions of the stars in space, relative to the earth, or if we prefer, to the sun. The sun moves about 12 miles per second, and other star velocities range from 5 to 100 miles per second.

A large number of true space motions were computed a few years ago by Strömberg, with the result of revealing a great common highway, along which not only the various groups of stars of different spectral types, but the great star clusters, and even the spiral nebulæ, seemed to be moving with reference to our sun. The phenomenon has been compared by Adams to the flights of a number of swarms of bees, which, fortuitously, had chosen a common path for their migrations. Individual swarms would fly at unequal rates, and individual bees at cross directions to the common course, so that a wide dispersion of swarms and individuals would appear. Yet on the whole the common course would predominate.

In order to understand this majestic march of the celestial hosts, we must first review the recent studies by Hubble of the spiral nebulæ and their relation to our Galaxy of stars. Hubble has been able, in photographs by the 100-inch reflector, to resolve several spiral nebulæ, including the Great Nebula of Andromeda, into hosts of separate stars of excessive

faintness. Some of these stars he finds to be of the Cepheid type of variable brightness. Shapley had shown that these variables are standard objects whose periods of variation are truly indicative of their absolute brightness. Hence Hubble was able to determine, from the periods of variation, the real brightness of these Cepheids found within the spiral nebulæ. This done, their distances followed at once, from a knowledge of their apparent magnitudes.

He found the distance of the Great Nebula of Andromeda to be about 900,000 light years, and some of the other spirals to be nearer, some more distant. Thus these objects lie quite outside the limits of our Galaxy of stars. They are, as Herschel suggested, Island Universes. Their shapes, as photographed in some cases on edge, suggest the configuration of our Galaxy. Their sizes, in some instances at least, are not dissimilar.

Hence we are led to suppose that our own Galaxy is a rather more than usually large and mature spiral nebula, in which almost all the nebulous matter has been agglomerated into stars. Its age is so great that the stars of different masses and different ages have taken on very unequal rates of motion.

Originally, as we may suppose, our own nebula, like all others, traveled in a certain direction with respect to the general algebraic zero of motion of all spiral nebulæ, at the rate of several hundred kilometers per second. This direction happened to be what we observe now as the great common star-way. Our own star, the sun, happens to be moving nearly along the great common way, and as its motion exceeds the average rate of some star groups, it is imparting to them an apparent negative velocity. A still higher apparent negative velocity equal to the real velocity of our own system is imparted in the same manner to the algebraic zero of motion of the other spiral nebulæ. Such is an explanation of the grand parade of the celestial hosts.

5. We have been dealing hitherto with gigantic things. Billions of stars, millions of degrees of temperature, sextillions of miles of distance, trillions of years of time, the march of the celestial hosts—all these tremendous facts have been on review

before us. Is it not, then, the height of anticlimax to turn away, at the ending, to consider the affairs of the solar system; ruled by that middling star, our sun, and comprising no other objects which could so much as be discerned by any intelligent dwellers among the nearest of the other stars?

It cannot appear so to one who is familiar with the intricacies of the chemistry and the physics, the marvels of the instincts and the adaptations and the triumphs of the infinitesimal, by which, for instance, the characters of a mature man are controlled by the constitution of the two microscopic germ cells which united to create him. These occur only in life, and life, so far as we yet positively know, occurs only in the solar system. It is not therefore so much the academic astronomy of the sun and planets, as their relations to life, which now I wish to touch upon in closing.

Although life on other planets than the earth is not positively demonstrated, there are two others, Mars and Venus, on which its existence is not out of the question. Yet life depends upon a nice adjustment of various conditions. There is no chemical element except carbon which combines with other elements in that infinite variety of potent forms so indispensable to the mysterious processes of life. Flexibility, too, is requisite, so that liquids rather than solids must have a considerable part in living creatures. Of all liquids water is the most important, but even of the numerous organic carbon-built liquids, those more conducive to life properties solidify as water does at about zero centigrade. These considerations incline us to think that planetary temperatures about as high as the earth's are requisite at least to the higher forms of life.

It has recently been observed that the illuminated side of Mars reaches equatorial temperatures approximating those of our spring days in Philadelphia. Both oxygen and water vapor have been demonstrated in the atmosphere of Mars, but in comparatively minute quantities. Adams and St. John find of oxygen 15 and of water vapor 5 per cent. of the quantities prevailing in our atmosphere. So the Martian life, if it exists, must be adapted to atmospheric composition approximating that high above the summit of Mount Everest.

As the atmosphere of Mars is so very rare and dry, it is unsuitable to retain heat at night. Computation and observation unite in estimating the midnight temperatures of equatorial Mars as of the order of -40° C. These frigid night temperatures, combined with the rare and dry atmosphere, would seem to exclude from Mars the higher types of life, such as we know, but might permit certain arctic types to exist. Indeed, the seasonal changes of color which are observed, seem to many to be satisfactory evidence of vegetation on Mars.

Upon Venus there is no defect of temperature, or of the uniformity of it. With greater nearness to the sun but higher reflecting power, the solar radiation available to warm Venus is about 1.4 times as intense as that which warms the earth. Accordingly, temperatures approximating those of our tropics should prevail in latitudes well towards this planet's poles. An abundant atmosphere is present. The reflecting power approximates that of a completely cloudy earth, so that it would be reasonable to conceive of clouds of water, completely hiding the planet's surface at all times.

The spectroscope, however, does not confirm this. Neither water vapor nor oxygen can certainly be discerned thereby. Yet it seems incredible that we see the surface of this planet, whose bulk must be solidified since its density is nearly the same as the earth's. For if solid, surely it would present some visible markings, and Venus never does. Accordingly it is supposed that the clouds of Venus are of the high-level cirrus type, and that water-vapor, though present plentifully below the clouds, is too scanty at higher levels to be revealed. As for oxygen, though certainly it cannot extend as high as it does above the earth, it may be present beneath the cloud-level.

No spectroscopic evidence of the rotation of Venus has ever been found. This proves that the planet does not rotate very rapidly, like the earth or Mars. It has even been suggested that, like the moon, its rotation and revolution periods are identical so that Venus would present the same face to the sun at all times. If that were so, the bright face would be very hot

and the dark face very cold. Recently, however, Pettit and Nicholson have found that the dark side of Venus is about equally warm from one edge to the other and is everywhere at about the same temperature that our earth would appear if viewed from another planet. This observation of moderate and equable night temperatures proves that the planetary rotation must be fairly rapid, and certainly not of the same period as the revolution. We may therefore conclude that Venus is very probably appropriately provided with temperature, humidity and atmospheric conditions, and is in a state suited for luxuriant life. Being wholly cloudy, however, it is doubtful if we can ever demonstrate it.

The earth, therefore, still remains the only known abode of life, and her life depends absolutely on the sun's radiation. Recent studies have shown that this dependence rests on very narrow margins of safety. For instance, the oxygen of the upper atmosphere is induced to combine into the form of ozone by the influence of extreme ultra-violet solar rays, and yet the ozone formed is continually being reconverted into oxygen by the influence of still other extreme ultra-violet solar rays. Thus occurs a balance of these effects such that the upper atmosphere contains so minute a quantity of ozone as would make, if brought to earth, a gaseous layer only as thick as a cardboard. Yet this minute and almost fortuitous atmospheric constituent cuts off entirely the spectrum of the sun and stars beyond wave-length 2,900 Angstroms. The solar rays thus cut off, if they reached the earth, would destroy human sight and tissues by their powerful chemical activity. Of course we could shield ourselves from these effects, but our ancestors who lived before the invention of spectacles would have lost their sight, or never attained it. Yet if the atmospheric ozone absorption reached only a little further, to 3,200 Angstroms, human and animal young would languish with the enfeebling disease of rickets, for the extreme ultra-violet rays are indispensable to proper mammalian growth.

As ozone absorbs to some extent even as far as 3,200 Angstroms, a catastrophe might ensue if circumstances led to a

small increase of the ozone content of our atmosphere. For this would eliminate indispensable rays of wave-lengths 2,900 to 3,200 Angstroms. Such a catastrophe would attend such an alteration in the distribution of the extreme ultra-violet solar spectrum as would change materially the existing balance of influences tending to produce and destroy ozone.

This leads us to inquire if the sun is a constant star, and, if not, what is the character of its variation. The fluctuations of sunspots and other visible solar phenomena which have long been known, prove, of course, that the sun is not absolutely constant. Within the last 22 years the Smithsonian Institution has made some thousands of determinations of the intensity of solar heating, which prove that the solar radiation increases several per cent. at times of maximum visible solar activity. Apparently, too, the sun's surface presents appreciable inequalities of radiating power, so that the rotation of the sun leads to successive brief changes of the intensity received at the earth's surface. These changes are slight for red and infra-red rays, but grow more and more considerable for the shorter wave-lengths. Pettit, indeed, observing with the narrow band of ultra-violet rays which silver transmits, centering at 3,160 Angstroms, finds alterations of over 100 per cent. in their intensity. This means that if our eyes were sensitive to such rays alone, we should find the sun's surface twice as bright at some times as at others.

Other solar phenomena exhibit interesting variations. Nearly 20 years ago Hale discovered magnetism in sunspots, and later over the sun's whole surface. Sunspots are apt to go in pairs, and Hale finds that if the advancing spot of a pair is a north pole in the northern solar hemisphere, it will be a south pole which leads in a spot pair in the southern hemisphere. But this state of things endures only through one eleven-year sunspot cycle. In the following cycle these polarities reverse, so that 22½ years are required to bring back the magnetic conditions to the starting point. Bjerknes has proposed an ingenious hypothesis to account for sunspots, their coolness, their going in pairs, the opposite magnetic po-

larity of the pair, and the reversal of polarity at each eleven-year cycle. It depends on hydrodynamic principles and explains the phenomena as due to causes residing within the sun, not to gravitational influences of the planets.

It is well known that terrestrial magnetism reacts to solar activity, and so does the aurora as well. Bauer has shown that the earth's magnetic state marches closely with the intensity of solar radiation as measured by the Smithsonian Institution. Very recently Austin has found that the reception of long-range radio signals also marches hand in hand with the intensity of solar radiation.

The weather, too, so important to human concerns, seems to be affected by solar changes. The importance of this effect is still in controversy, so that I shall not stress it, but merely remark that time will tell. However, I must point out that the solar variation, though obviously associated with the 11-year sunspot cycle, has hitherto seemed irregular, and therefore unpredictable. But now there seem to appear definite periodicities of 25⅔, 15 and 11 months, and certain harmonics of these periods, which, together with the eleven-year period, seem to make up the whole long-interval solar variation. If these definite periodicities shall persist, we shall be in position to forecast for years in advance the principal solar variation, and everything which may be found to depend upon it.

I should give but a feeble impression of the importance of sunlight to life if I should stop at this point. All plants grow by absorbing solar energy and using it to promote chemical reactions in a way still inimitable by chemists. Ultra-violet rays, too, produce certain changes of chemical structure in fats and oils which are the source of those traces of hormones so extraordinarily important, all out of proportion to their infinitesimal occurrence, in the growth and health of animals. The more searching study of the solar spectrum in its relations to these extraordinary chemical reactions is a most fascinating field.

On the grosser side, the application of solar energy to

power production, also offers an attractive research field. Some are disposed to think this chimerical. There are, however, certain lines of improvement over former attempts at the utilization of solar rays directly for power which, I am inclined to think, will solve the problem commercially.

Thus far we have mentioned only points of contact between astronomical research and physical life. Yet this aspect, interesting as it is, should yield place to appreciation of the value of astronomy to broaden the mental outlook, to vanquish fear, and to allay superstition. While yet the vast numbers and masses of the stars were unknown, their nature unrecognized, the existence of other Galaxies undreamed of, men very naturally regarded the earth as the central object and themselves the beings for whom all things were created.

Now the universe has attained such grandeur that earth sinks to the dimensions of a speck, and we cannot but accept an attitude of wholesome humility. At the same time, the consciousness of having achieved already so great a command of the forces of nature, and so considerable a knowledge of her mysteries, and the assurance that we can soon command and know far more, tempers humility with elation.

PERSISTENT GENERATION OF HEAT IN SOME ROCKS AND MINERALS; AND ITS PROBABLE SIGNIFICANCE

Second Paper

By CHARLES F. BRUSH

(*Read April 29, 1927*)

At the General Meeting of this Society in April, 1926, I had the honor to present a paper under the title "Discussion of the Kinetic Theory of Gravitation, III. Some Experimental Evidence Supporting Theory; Continual Generation of Heat in Some Igneous Rocks and Minerals. Relation of this to The Internal Heat of The Earth and Presumably of The Sun." [1]

In that paper the writer's isotropic ether-wave theory of gravitation announced in 1910 was briefly reviewed, and the generally accepted kinetic theory of heat was concisely stated. Quoting from the paper:

> From the fortuitously wide variation in velocities and free paths of the billions of vibrating atoms or molecules in their heterogeneous movement, it follows that collision frequencies must also vary greatly, from instant to instant, everywhere in a body of matter.
>
> Probably the postulated gravitation waves are not confined to one frequency, but have a wide range of frequencies as do the well-known X-rays.
>
> With the foregoing in mind it is easily conceivable that some kinds of matter may have atoms or simple molecules or complex molecules of occasional vibration frequency corresponding with some gravitation wave frequency, whereby fortuitous resonance can, for brief instants, be established at various points. This would result in a slight increase of vibrational activity and a cumulative rise of general temperature, perhaps sufficient to be detected.

[1] *Proc. Am. Phil. Soc.*, Vol. LXV., No. 3, 1926.

A body of such matter, with some thermal insulation, would become and remain permanently warmer than a neighboring body similarly circumstanced, but not endowed, or less endowed with the permissive heat-generating quality.

Then follows the account of a long search for such matter, culminating in the discovery that some basalts, lavas and minerals, crushed to coarse sand size, generate considerable heat. A diagram and description of the calorimeter used was given. Quoting again from last year's paper:

The computed rate of heat generation in the Lintz basalt, 3° C. in a year, is extremely large; and it is thought that some or much of it may be due to the very large area of new surface created by crushing, and the severe shocks throughout the grains incident thereto, whereby a slow rearrangement or incipient crystallization and regrouping in the surface layer of molecules and even in the grain mass of this complex rock may occur with some generation of heat, analogous to the spontaneous generation of heat in recently hardened steel. Doubtless basalts and lavas are particularly susceptible to such action because they are in a condition of suspended crystallization arising from their comparatively rapid cooling after ejection.

If the suggested action occurs, several days, weeks, or even months may be required to reach entirely stable condition. This very important point will be determined by retesting after a lapse of several months.

The suggested action appears to have occurred in part; and this is a very interesting discovery. It probably explains why Pool's work with pitchblende, hereafter referred to, gave at first, heating effects much larger than some months later. The paper when published contained an important "Supplement" written three months after presentation, briefly detailing the results of retesting some of the materials after intervals of several months. In every instance a decline in the rate of heat generation was found; in some cases very large.

During the last two months I have made a third test of some of these materials after a second interval of many months; also second tests of some others after long intervals. Always the same amount of the same lot of material was used

in the first, second and third tests. Following is a résumé of these tests:

The Utah lava (Scoria) had lost largely in its heat generating activity during the 9½ months between its first and second tests. In the next 8 months (third test), it had lost about 25 per cent. of its second test activity.

The Otumba Lava (Scoria) which had lost largely in activity during the seven months between its first and second tests, suffered no further loss in activity during the next 8½ months (third test).

The hard burned Sandusky Clay lost largely in activity during the 8 months between its first and second tests, but suffered no further loss in activity during the next 8 months (third test).

All the above tests were made with the "old calorimeter" described in the last paper.

The following tests were all made with the "ice calorimeter" described later.

Sandusky Clay, gently ignited, first tested 9 months after its preparation, gave rather large heat generation; but after another 8 months this had declined fully half. Probably, soon after preparation, this specimen would have shown very large activity.

Sandusky Clay, washed and air dried at room temperature only, had considerable activity. After 8 months this activity had not declined in the slightest degree.

Quoting from the supplement of last year's paper:

> Another lot of Lintz basalt was crushed to very much coarser grain size than formerly, and all small grains and dust were removed by sifting. The coarse material was then maintained at a steady temperature of about 345° C. continuously for a full week, then slowly cooled. It is thought that during this long-continued moderate heating the basalt reached stable condition. Forty days afterward it was placed in the calorimeter and its rate of heat generation measured.

This exhibited moderate but very considerable heat generating activity. Recently, after 8½ months, it was tested

again. Its activity had *more than doubled;* showing that the long heat treatment had been detrimental. Probably a much shorter heat treatment would have been sufficient to effect stable and permanent condition, with perhaps greater activity than last shown.

The above synopsis of tests indicates that the Otumba Lava; the hard-burned Sandusky Clay; the air-dried Sandusky Clay, and the Lintz basalt have almost certainly reached stable condition. All manifest very considerable heat generating activity, and I see no reason to doubt that this will persist indefinitely. It is my intention, however, to test some of these materials again after another year has elapsed.

The significance of persistent generation of heat in some rocks and minerals, as the sufficient cause of maintenance of the internal heat of the earth and other planets, and perhaps of the sun, was discussed in last year's paper.

Immediately following last year's meeting I made an important addition to the calorimeter then described (hereafter called "The old calorimeter") consisting in a reversing switch between the calorimeter and galvanometer.

Figure 1 is a diagram of the switch. It consists essentially of two blocks of well seasoned wood 8 x 11 x 3 cm. The upper block *A* has a central hole freely fitting the shank of a large brass screw firmly screwed into the center of the lower block *B* with its axis accurately normal to the surface of the block. Under the head of the screw is a flat brass spring adapted to hold the blocks together with a gentle flexible pressure when the upper block is turned on the lower. *C* is a handle for turning the upper block and indicating its position. The wires from the calorimeter are of copper 0.1 cm. diameter, heavily covered with rubber composition. Each insulated wire enters the block *B* near its lower side 2 or 3 cm., and from there on its insulation is removed and the bare wire extends upward to the face of the block, makes a right-angle turn,

thence along the face of the block, embedded half its diameter therein, 3 cm. to a second right-angle turn downward into the block as shown by the dotted lines. Thus the free end of the wire is secured against lateral displacement. *D* shows the upper face of lower block with its bared copper wires.

The wires from the galvanometer, after making several spiral turns to afford flexibility, pass downward through block

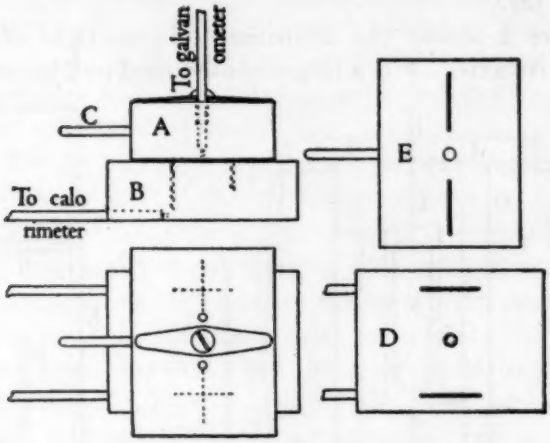


FIG. 1.

A, are bared, and pass 3 cm. along its lower surface, then into the block as described for block *B*. *E* shows the lower face of the upper block *A*.

When the handle *C* is turned from left to right, connection of the calorimeter with the galvanometer is reversed; and when the handle points downward the circuit is open.

The true galvanometer deflection is half the sum of the right and left deflections, irrespective of any small e.m.f. in the galvanometer structure, because this is automatically corrected.

The switch is attached to an inside brick wall, 2 m. from the floor, where temperature is very steady, and about 2 m. from the observer.

In long-continued use this switch has been found entirely reliable and satisfactory.

At the time of installing the switch just described, I built a second calorimeter (hereafter called "The ice calorimeter") somewhat like that used by H. H. Poole in measuring the rate of heat evolution due to radioactivity in pitchblende.[1] One end of a series of thermo-couples is maintained at the temperature of melting ice, while the other end is embedded in the material being tested, contained in a silvered Dewar vacuum jar.

Figure 2 shows the calorimeter in vertical cross-section through its axis. *F* is a large iron-hooped oak barrel having a

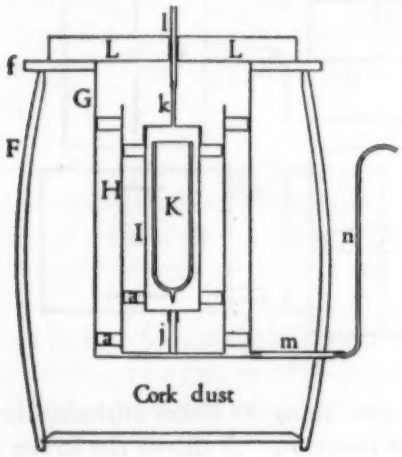


FIG. 2.

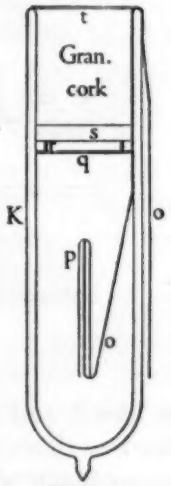


FIG. 2a.

square wooden cover *f* through which passes the open top of a cylinder of thick sheet copper *G*, closed water-tight at its lower end, 33 cm. in diameter x 64 cm. deep. The space between the cylinder and inside of the barrel is packed with cork dust for thermal insulation.

H is a second open-topped cylinder of thick sheet copper 21.6 cm. in diameter x 53.3 cm. deep. The bottom of this cylinder has several small holes for drainage into the outer cylinder and is raised slightly from the bottom of the latter by strips of wood.

[1] *Phil. Mag.*, Vol. XIX., p. 314, 1910.

I is a third cylinder of thick sheet copper 11.5 cm. in diameter x 39.5 cm. deep. To the bottom of this cylinder is soldered a brass ferrule holding a wooden post *j* which supports the bottom of this cylinder 9 cm. above the bottom of the second cylinder. The top of cylinder *I* is provided with a close-fitting copper slip-cap from which rises a small brass tube *k* prolonged upward by a rubber tube *l*. These tubes provide an exit for the fine silk-covered copper wires forming the terminals of the thermo-electric system within and outside of the Dewar jar *K*. The latter is supported in a light wooden rack inside the copper cylinder *I*.

Near the top and near the bottom of each of the cylinders *H* and *I* are four radial wings of sheet copper *aa* soldered to the cylinders and adapted to hold them in concentric position.

LL are two slabs of compressed cork each 53 x 24 x 5 cm. covering the calorimeter and preventing radiant heat from the room reaching the ice in the copper cylinders.

A small short brass drain tube is soldered to the side of cylinder *G* near its bottom, prolonged by a rubber tube *m* through the side of the barrel, thence upward by a glass tube *n* to a point such that the surface of the water from the melting ice in cylinders *G* and *H* shall be maintained as high as practicable without danger of flooding the cylinder *I*.

Figure 2*a* is an enlarged view of the Dewar silvered vacuum jar *K* and its appurtenances. This jar is 30 cm. deep x 7 cm. inside diameter.

The thermo-electric system is 603 cm. long and has one copper-constantan and three iron-constantan junctions at each end, all in series. In making each junction the covered wires were bared 1 cm., twisted together and soldered. After removing every trace of flux and working smooth, the junction was covered with insulation and shellacked. Finally all the wires were bound into a small cable *o* and shellacked.

One end of the thermo-cable *o* is located vertically in the axis of the cylinder of material to be tested, and covered by a glass tube *p*. From the lower open end of this tube the cable passes diagonally upward to the inside wall of the Dewar jar;

thence up the wall, over the top, and downward near to but not in contact with the outside of the jar, and ending near its lower part.

The fine silk-insulated copper wire outleads from the cable are taken off when the cable passes over the lip of the Dewar jar; from whence they pass upward through the tubes *k* and *l*, Fig. 2, to a reversing switch just like the one already described, and thence to the galvanometer. The circuits from both the "old calorimeter" and the "ice calorimeter" lead to the same galvanometer; but they do not conflict in any way because when one calorimeter is under observation, the circuit of the other is left open.

Returning to Fig. 2*a*: The test material, filling about two-thirds of the Dewar jar, is covered with a thick cardboard disc *q* which has been soaked in hot melted paraffin. Attached to the upper side of the disc *q* is a metal ring *r* forming an annular space next to the inner wall of the jar. The annular space is partly filled with stiff vaseline melted against the wall of the jar by a hot wire. Thus the test material is sealed in air-tight, except for small air-leak made by drawing, with a small needle, a pair of cotton threads through the disc *q*, and cutting off near each surface. This air-leak is to avoid breaking the vaseline seal by shrinkage of the air in the test material when cooled, and by changing barometric pressure.

Above the disc *q* and ring *r* is a thick disc of felt *s*, and above this the jar is filled with granulated cork. Finally, the jar is covered with a paraffined disc of cardboard *t* waxed on air-tight, but provided with a small air-leak like that of the other disc *q*.

The purpose of the disc *q* is to prevent migration of moisture between the test material below, and the felt and granulated cork above, which might otherwise follow unequal change in hygrometric condition due to lowered temperature.

With all parts in place as shown in Fig. 2, the annular space between cylinders *G* and *H* ("the outer zone") and between *H* and *I* ("the inner zone") and above both zones is filled with machine broken ice in small lumps from peanut to walnut

size. Planed or finely crushed ice is objectionable. Then ice-water is added until it overflows from the drain tube *n*. The deep space below cylinder *I* is provided to make sure that ice shall always be well below its lower end. This is essential, because water of abnormal density, and temperature above zero, tends to collect here (in the absence of ice) and must not be allowed to reach the lower end of the cylinder.

The broken ice for the top filling and for future replenishment, is always soaked in ice-water from six to ten hours before using. This is to bring the ice as nearly as practicable to zero temperature. Even after soaking, the lumps will freeze together more or less firmly when left dry. This is because the interior of even a small lump of ice is always somewhat below zero. In view of this it is customary to spray the ice in the top of the calorimeter several times daily with a small quantity of cold water to keep the surface of the lumps wet, and thus prevent the cap of cylinder *I* falling below zero temperature. (It is above the water level). This may be an unnecessary precaution because the high thermal conductivity of the cylinder tends strongly to preserve uniformity of temperature within it. However, if I were building another calorimeter I would provide means of capping the top of cylinder *I* water-tight to permit keeping the water level above it, and thus make spraying of the ice unnecessary.

Ice in the inner zone is poked down with a small iron rod every three or four days. Melting in this zone is very little indeed. The outer zone is poked down daily. Nearly all the melting occurs here, as should be expected. Ten Kg. of soaked ice every three days is ample to keep the calorimeter full. The small consumption of ice is due to the fact that the outer barrel is very nearly air-tight, which prevents leakage of cold air through the insulating cork dust.

In five or six days temperature inside the Dewar jar *K* falls sufficiently to permit observation of galvanometer deflections. Thereafter deflections are observed three or more times daily.

In about two weeks from the start galvanometer deflection becomes constant and remains so, provided barometric pres-

sure remains fairly steady. But if a large and rapid change of pressure occurs, this is followed a few hours later by a very noticeable change in galvanometer deflection. For instance: In a recent case, a fall of 7 mm. in a few hours was followed a few hours later by a very considerable drop in deflection, indicating a loss in temperature difference between the inside and outside of the Dewar jar of something like a thousandth of a degree. All this is clearly shown by comparison of the deflection curve with a barometric record covering the same period of time. In two or three days the former constant deflection was restored.

I attribute this phenomenon to the very small rise in melting temperature of the ice which the diminished atmospheric pressure must cause; whereby temperature of the outside, cold end, of the thermo-electric system rises faster than heat generation in the test material surrounding the warm end of the system, can compensate it.

The ice calorimeter has now been in satisfactory use about ten months, and its performance has been such as to inspire high confidence in the results obtained with it.

Both the "old calorimeter" and the "ice calorimeter" were used in making the tests outlined in the "Supplement" to last year's paper, and in the further very recent tests before referred to. Always the same calorimeter was used in the first, second and third tests.

It is notable that all the materials which appear to be endowed with persistent heat generating activity are very complex silicates. And it seemed highly probable that some silicates may be very much more active than others. This now appears to be true.

With this thought in mind I commenced last summer a systematic campaign with artificial silicates. This is still in progress.

Many silicates have been prepared by precipitating dilute solutions of the sulphates of certain metals, one molecule of each, with dilute solution of sodium silicate in slight excess;

boiling several hours to insure complete reaction; washing the precipitate, usually by many decantations, until soluble matter (mostly sodium sulphate) was reduced to a very small fraction of one per cent.; draining on paper filters and drying in a hot air bath until they ceased to lose weight.

All the silicates thus prepared were very voluminous, easily crushed between the fingers, light in color and rather hygroscopic. Half of each (there was enough for two experiments) was gently ignited by slowly heating to about 540° C., maintaining at that temperature two hours and slowly cooling.

All (containing more or less of the iron group of metals) darkened very much in color, lost little if any in weight and became only slightly hydroscopic. They gained very little in density and were still easily crushed between the fingers. There was no evidence of any sintering.

The sodium silicate used in precipitating the silicates contained, by analysis, 1 part by weight of Na_2O, to 3.3 parts SiO_2:

If the silica simply displaced the sulphuric acid of the bases, the precipitated silicates would have an easily computed weight. But they all weighed much more than this, indicating that from 1/4 to 1/2 a molecule of sodium silicate remained in insoluble combination with each molecule of the precipitated silicate.

The first lot of artificial silicates contained the light metals of low atomic weight, aluminum, magnesium, beryllium and some lithium; with iron (ferrous) nickel, cobalt and manganese. The second lot contained only the iron group of metals; iron (ferrous) nickel, cobalt and manganese. The third lot was like the second with omission of the iron. The fourth lot contained only nickel and cobalt. Three other lots contained nickel, cobalt and manganese singly.

All of the artificial silicates have been tested once in either the first, or the ignited condition, some in both conditions. All have shown good heat generating activity, some very large. The latter was particularly the case with the nickel-cobalt combination, ignited, which had an activity something like ten

times greater than the most active material cited in last years' paper.

Second and third tests of the most active silicates will be made after many months, to observe decline in activity, if any, and the results will be embodied in a future paper.

In the absence, at present, of other explanation, it is thought that persistent heat generation in some rocks and minerals is due to isotropic ether waves of great penetration; very great indeed, if the generation goes on in the interior of the sun and planets as it does at the surface of the earth.

Of course, if this be true, it does not prove that the waves are the same as those postulated as the cause of gravitation, but it is thought to be a very strong affirmative argument.

As I have pointed out before there can be no doubt that interstellar space is filled with approximately isotropic waves of radiation emitted from millions of very large and brilliant stars. And these waves must have at first a much wider range of frequency than those from the sun, because a large proportion of the stars are very much hotter than our sun. And now Dr. Millikan tells us of "cosmic rays," doubtless isotropic, and of vastly greater frequency and penetration than any heretofore known, and these make a most important addition to the known isotropic ether waves of interstellar space, and, I think, constitute a long step toward the postulated isotropic ether waves causing gravitation.

There appears to have been long prevalent a general impression that a lump of matter, say a piece of sandstone, generating no heat in itself, would, if placed in interstellar space, soon fall to absolute zero of temperature by radiating all its heat; in other words, that the temperature of interstellar space is absolute zero. Of course, radiation is not temperature, and there can, strictly speaking, be no temperature where there is no matter to manifest it. But the term "temperature of space" is a very convenient one and perfectly clear in its meaning, so I shall use it. It means the temperature which the piece of sandstone would permanently maintain.

As a necessary consequence of the supposed absolute zero of stellar space, many writers appear to have taken for granted that all the heat of the earth's surface and atmosphere, except the small amount slowly conducted through the earth's crust from its hot interior, comes by radiation from the sun; that the sun is the source of virtually all the heat which maintains the earth at habitable temperature.

I think both the above hypotheses are untenable in view of the certainly considerable amount of radiation everywhere in stellar space.

I venture the opinion that a lump of matter, or a body of gas anywhere in stellar space, would acquire a permanent temperature something like 50° or possibly even 100° C. above absolute zero.

This view appears to be strongly supported by several well-established natural phenomena: (1) The "inversion layer" or "stratosphere" of the earth's upper atmosphere. In this connection I shall quote from Dr. Humphreys' "Physics of the Air."

Our atmosphere has been explored to a height of about 35 kilometers, say 20 miles, by means of "sounding balloons" carrying several recording instruments. Observations have been carried on for many years, winter and summer, in many places in various latitudes. But the results are always substantially the same. The temperature of the air falls rapidly until a height of about 11 kilometers is reached; then a rather abrupt change in the temperature curve takes place. This marks the lower surface of "the isothermal layer" or "stratosphere" of the atmosphere. The height of this lower surface varies considerably in different seasons of the year and in different latitudes. But in the stratosphere the temperature of the air remains substantially constant at all heights; even tending to rise slightly as height increases. Dr. Humphreys finds the average summer temperature of the stratosphere at a height of 20 Km. (about 12½ miles) above sea level, to be about −49° C.; and the winter temperature at the same level to be about −58° C., or 215° C. absolute.

Dr. Humphreys (and almost simultaneously Dr. Gold in England) has given us a beautiful theory to account for this remarkable phenomenon. But I venture to suggest that it may be considerably due to the "temperature of space," supplemented by radiation from the earth's surface as Dr. Humphreys has so well explained.

(2) The mild winter climate of the Arctic regions. The lowest temperature I find recorded by Nansen was −50° C. (−58° F.) on Feb. 19th, 1894, in latutide about 80°, far from any land. The lowest I find recorded by Peary was −53° C. (−63.4° F.) on Dec. 19th, 1908, at Cape Sheriden in latitude about 82°. In both cases the sun had been continuously below the horizon for several months, affording no heat from that source; the winds were light, and the sky was clear. Doubtless there was very little water vapor in the air, thus affording most favorable radiation into space.

Under such conditions we should, I think, expect *very* much lower temperatures than those recorded, *unless* outgoing radiation were considerably compensated by incoming radiation from space. We should also expect formation of very much thicker ice in the polar seas during the long winters, were it not for the warming effect of the "temperature of space."

(3) The surprisingly mild climate of the planet Mars. It appears to be the general consensus of opinion among astronomers that the white polar caps of Mars consist of frozen water, like the polar ice and snow caps of the earth. The ice or snow caps of Mars cover a much larger area in the Martian winter than in the Martian summer; just as is the case with the earth's polar ice and snow caps. Lowell estimates the temperature of the warmer surface of Mars to be about 48° F. Surely the equatorial and temperate zones of Mars must have a temperature something like this, because snow does not form there, and there is some color evidence of vegetation along the water courses, or "canals," in the Martian summer season.

Presumably the interior of Mars is very hot, and his surface must derive some heat from that source; but probably not so much as does the earth, because his ratio of mass to surface is much less.

The average climatic temperature at Cleveland, Ohio, where this paper is written, has been 49.2° F. for the last 55 years (U. S. Weather Bureau). This happens to differ only about one degree from Lowell's estimate for the warmer surface of Mars. Cleveland is about 200 m. above sea level, and in about mid latitude of the North temperate zone. The mean temperature of the whole earth's surface has been estimated at 287° absolute (14° C.), which is not much above Lowell's estimate for the warmer parts of Mars. *And yet Mars receives considerably less than half as much heat from the sun, per unit of surface, as does the earth.*

How, then, can we account for the mild climate of Mars? I beg to suggest again that it is due to the "temperature of space."

The temperatures quoted in the foregoing are somewhat confusing, because they refer to both Centigrade and Fahrenheit scales, which have quite different degree values, and are plus or minus with reference to a conventional zero which is quite different in the two systems. To facilitate easy comparison of the temperatures and indicate their real quantitative values, I have reduced them all to the Centigrade scale, starting from the theoretical absolute zero:

Melting ice	273°
Stratosphere, average summer	224
Stratosphere, average winter	215
Arctic winter, lowest by Peary	220
Planet Mars, by Lowell	282
Cleveland, average for 55 years	283
Earth's surface, average	287
Temperature of space, rough estimate	50 to 100

The question naturally arises, how can the feeble general radiation from the stars account for anything like in amount to the supposed temperature of space? Would it not require that the night sky be so brilliant with star light as to be comparable in brightness with the face of the moon? In this connection my line of thought is indicated in the following tentative explanation:

Astronomers tell us there are a great many vast regions of space filled with highly attenuated cosmic matter; the stuff of

which nebulæ are made; the stuff of which nebulæ are doubtless now in the making, but not yet sufficiently condensed and hot to be luminous. The cosmic matter visibly intercepts some of the light from stars beyond it, and is sufficiently dense in spots to quite obliterate faint stars. Intercepted radiation from stars in all directions must tend to raise the temperature of cosmic matter. And we must not forget that only a part of stellar radiation is of such wave-length as to be visible. Doubtless a part of the continually growing heat energy of the cosmic matter is retained by it, and a part is radiated into space in long wave-lengths.

Astronomers also tell us there are a great many dark bodies in space; probably former stars so far cooled by radiation as to be non-luminous. These also must be contributing long wave-length radiation to space.

It would seem then, that long wave-length radiation in space is a cumulative phenomenon; and accumulation has been going on for untold millions of years. May it not now be sufficient to account for the supposed "temperature of space"? A quotation from my first paper on gravitation [1] seems appropriate:

> We are familiar with the dissipation or degeneration of the higher forms of energy into heat, and the continual degradation of heat to lower degree; that is to say, less violent molecular vibration and more general distribution. As is well known, it is only through this degradation or running down of natural energy that we are enabled to utilize some of it. Lord Kelvin called this function of energy "Motivity" (we now call it entropy) and said the motivity of the universe tends to zero.
>
> We know that ordinary radiation waves in the ether persist indefinitely and without change of frequency or direction until they encounter matter, when they are absorbed and converted into heat, only to be radiated again, usually in longer waves, to some colder body. This degradation of wave frequency continues until we can no longer follow it. I beg to suggest that the ultimate destination of this wave energy is that vast reservoir of kinetic energy intrinsic to the ether.

CLEVELAND,
April, 1927.

[1] *Science*, March 10, 1911; *Nature*, March 23, 1911.

THE WIDMANSTÄTTIAN STRUCTURE OF IRON ALLOYS

By ALBERT SAUVEUR

(*Read April 29, 1927*)

When rightly interpreted, the Widmanstättian type of structure so frequently observed in alloys of iron with other elements throws a great deal of light on the mechanism of the changes of structure which occur in these alloys as they cool from their solidification temperatures to room temperature.

Broadly construed, a Widmanstättian structure indicates the breaking up on cooling of a solid solution through a new phase coming into existence and locating itself at the boundaries of the original crystalline grains and along their crystallographic planes.

In the case of iron alloys, and confining our attention to binary alloys, at a high temperature they consist of a solid solution of a metal or a metalloid in gamma iron. When the dissolved element is carbon, the solid solution is universally called "austenite." Other elements, however, may be dissolved in gamma iron, and we may talk of manganese austenite, nickel austenite, etc. When austenite is allowed to cool slowly to room temperature, it may, according to its chemical composition, either remain undecomposed, retaining the polyhedral structure common to all solid solutions, or it may break up into two constituents, which breaking up implies the formation of a Widmanstättian structure. Let us first consider the transformation of the pure solvent, that is of pure gamma iron.

Pure Iron

Pure iron above its allotropic transformation point, A_3, consists of grains of gamma iron (Fig. 1) each made up of

small octahedral crystals necessarily of uniform crystalline orientation in the same grain but of different orientations in adjacent grains. On cooling through the point A_3, at about 900° C., iron passes from the gamma condition to a new allotropic form generally designated as alpha. This transformation, however, is not instantaneous. There are good reasons for believing that it begins at the grain boundaries and along the octahedral planes of the crystals and that it progresses until each grain of gamma iron is changed into a grain of alpha iron the resulting structure being similar to that of gamma iron (Fig. 1). Were it possible to retain at room temperature pure iron but partially transformed, it seems probable that a Widmanstättian structure, or rather, as later explained, a "martensitic" structure would be observed (Fig. 2), consisting of alpha iron at the boundaries and at the octahedral planes, and of a groundmass of gamma iron. It is also probable that such partially transformed iron would be found considerably harder than alpha iron, not because of a greater hardness of the gamma phase, but because of the inherent hardness of a martensitic structure. It is not, however, possible, even through very rapid cooling, to prevent in pure iron the complete transformation of gamma into alpha iron and the formation of a polyhedral structure similar to that of gamma iron (Fig. 1). This is probably why pure iron cannot be hardened by quenching.

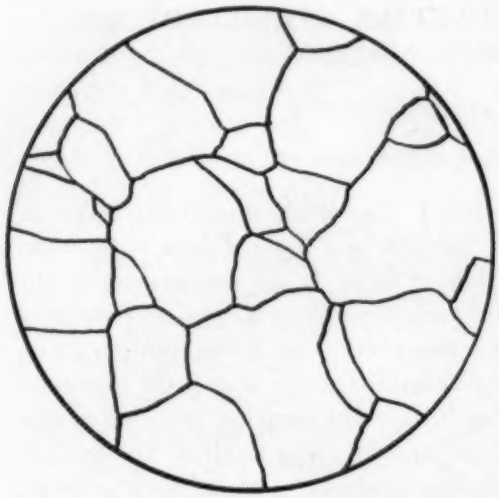


FIG. 1.—Schematic illustration of the microstructure of gamma iron, of ferrite (alpha iron) and of austenite.

Alloys of Iron and Carbon

Assuming iron containing some 0.50 per cent. carbon, such alloy consists above its thermal critical range, like all other iron-carbon alloys of the steel series, of a solid solution of carbon, or of the carbide Fe_3C in gamma iron. As already noted these solid solutions are called austenite. They are, like pure iron, made up of polyhderal crystalline grains, each

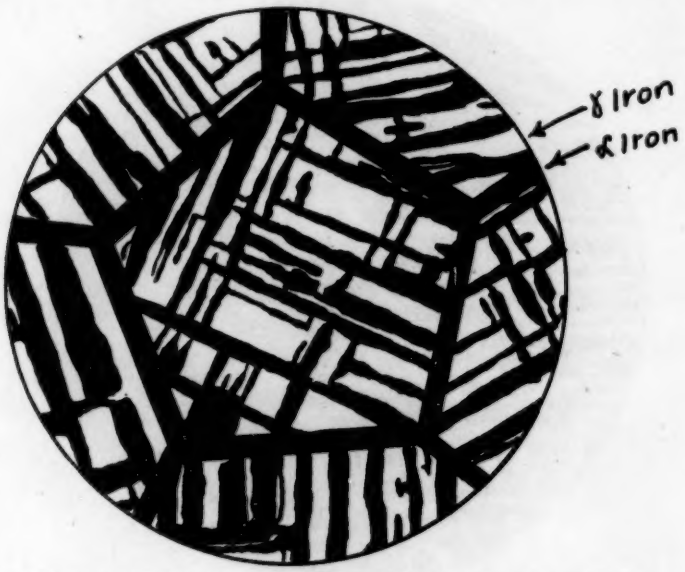


Fig. 2.—Schematic illustration of the Widmanstättian type of structure assumed by pure iron in passing from its gamma to its alpha condition.

one consisting of many small octahedral crystals of uniform orientation in any one grain but of different orientation in any two adjoining grains (Fig. 1). When this alloy cools slowly through its A_3 or A_{32} point, at some 750° C., the gamma iron it contains gradually transforms into alpha iron. While this allotropic transformation begins at the upper point, it is not complete before the alloy has passed through its A_1 point, in the vicinity of 700° C. Let us examine the mechanism of that transformation, as revealed to us by the Widmanstättian type of structure exhibited by the alloy after slow cooling (Fig. 3).

On reaching the upper critical thermal point the gamma-alpha transformation begins at the boundaries and proceeds along the octahedral planes of the crystalline grains. The new phase (alpha iron) now making its appearance has very little, if any, solubility for carbon, which is therefore expelled into the gamma phase, the latter becoming progressively richer in that element. Assuming this elimination of carbon

FIG. 3.—Widmanstättian structure of medium high carbon steel after slow cooling to room temperature, magnified 100 diameters. The white constituent is ferrite (alpha iron), the dark, pearlite.

by the alpha phase to be complete, pure alpha ferrite is found located at the boundaries and at the crystallographic planes (Fig. 3). As previously noted, this allotropic transformation of gamma iron into alpha iron and accompanying elimination of carbon from the alpha phase proceeds with falling temperature (from A_3 to A_1), until at A_1 the remaining gamma phase now contains some 0.85 per cent. carbon in solid solution, when on passing through the A_1 point it breaks up into pearlite, that is into a fine eutectoid aggregate of alpha iron

(ferrite) and the carbide Fe_3C (cementite) (Fig. 3). This completely transformed alloy is considerably harder than pure (alpha) iron, (1) because of the hardness of the cementite it contains, (2) because of the fineness of the structure of pearlite, and (3) because of its Widmanstättian type of structure.

It is possible by rapid cooling to prevent the complete transformation of gamma iron into alpha iron and therefore

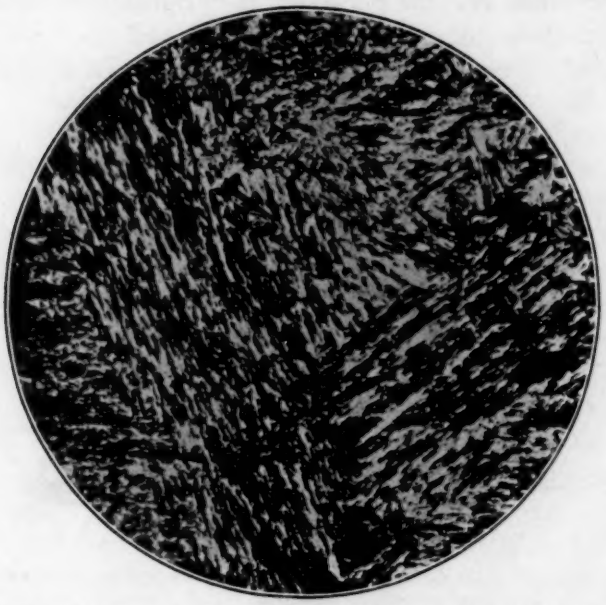


FIG. 4.—Martensitic structure of medium high carbon steel quenched in water from a temperature exceeding its thermal critical range. Magnified 500 diameters.

to have at room temperature both some of the gamma and some of the alpha phases. This explains the hardening of steel by quenching. Let this be clarified. By rapid cooling, the gamma-alpha allotropic transformation is delayed until a considerably lower temperature is reached (some 300° C.), when less time is also afforded for its completion. Under these conditions some of the gamma phase (austenite) is retained. The structure is still of the Widmanstättian type, but much finer and is then called martensite (Fig. 4).

In the constituent of hardened steel generally known as

martensite, both the alpha and the gamma phases are therefore present, while in the Widmanstättian structure, resulting from slow cooling, the gamma phase no longer exists. This may serve to distinguish between the two structures, bearing in mind that the mechanism of their formation is otherwise similar.

In a martensitic structure the alpha phase is to be found in the crystallographic planes of the original austenite grains and at their boundaries, but it has not had the time to grow into the thick and large plates observed in the Widmanstättian

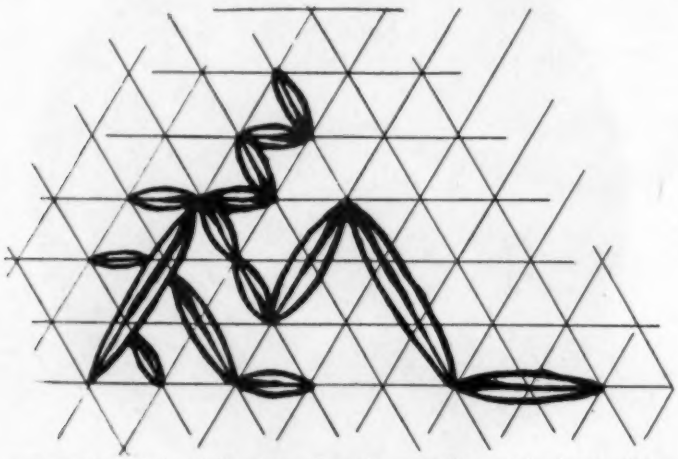


FIG. 5.—Formation of the alpha phase along the crystallographic planes of the octohedral crystals of austenite. (F. F. Lucas.)

structure resulting from slow cooling; hence the much greater fineness of the martensitic structure. The alpha phase present in martensite, moreover, contains some carbon in solution, the time necessary for its complete elimination having been denied. This alpha phase, as Benedicks remarks, is a supersaturated solution; hence its hardness. It is generally called troostite but is described by most as a finely divided aggregate of alpha iron and cementite. The development of the alpha phase is clearly shown by Lucas in Fig. 5.

The gamma phase, or austenite, forming the ground mass of the martensitic structure is, following Benedicks, a super-

cooled solid solution of carbon in gamma iron. It might be more accurate to say that it is the gamma iron it contains that is supercooled, rather than the solid solution itself, as there is no reason to believe that gamma iron when retained at room temperature loses its solvent power for carbon. Because of the supercooled condition of gamma iron at room temperature, it is probable that its solution with carbon is then harder than it would be if gamma iron was normally stable at that temperature.

Summing up, the hardness of martensitic steel is due primarily (1) to the hardness inherent to the martensitic type of structure, (2) to the fineness of that structure, and (3) to the hardness of both the alpha and the gamma phases.

Since gamma iron at room temperature is supercooled, it implies a tendency on its part to revert to the alpha phase, which is the stable one at that temperature. The reheating of hardened steel, generally called "tempering," makes possible this additional transformation of some gamma into alpha iron, which implies the formation of more troostite. The alpha phase, troostite, moreover, being supersaturated, tends to expel carbon in the form of minute particles of the carbide Fe_3C, and this expulsion is made possible by reheating as in tempering.

It is believed by some that the unstable condition of hardened steel—due, in the writer's opinion to the supersaturated state of its alpha phase and the supercooled condition of its gamma phase results in some finely divided particles of cementite being formed even on aging at room temperature. According to Jeffries and others, this dispersion of very small hard particles actually increases at first the hardness of freshly formed martensite. As they increase in size, however as in tempering, the hardness of martensite begins to decrease.

In order that iron may be substantially hardened by quenching it from a high temperature, it must be alloyed with an element or chemical compound of limited solubility in gamma, while being insoluble or nearly so in alpha iron, and present in such proportion that on rapid cooling a considerable

amount of gamma iron will be converted into alpha iron at the boundaries and along the crystallographic planes of the original gamma grains, giving rise to a martensitic structure. Should the solution remain completely untransformed, it would assume a polyhedral structure, would be free from alpha iron, and would not be very much harder than the solvent iron. If, on the contrary, the transformation is complete, an aggregate would result, the hardness of which would depend upon the quantity, size, and mode of distribution of particles of the solute.

It is hardly to be expected that carbon alone possesses the necessary requirements to impart hardening power to iron. Other elements or compounds must exist, which like carbon are more soluble in gamma than in alpha iron and capable therefore of inducing martensitic structures on rapid cooling, and hence of greatly increasing hardness.

It is probable that elements having the same space lattices dissolve in each other more readily than those having more dissimilar crystal structures. Following this line of thought, it is noted that gamma iron has a face-centered cubic space lattice, whereas alpha iron has a body-centered lattice. Elements having a face-centered space lattice should therefore be more readily soluble in gamma iron than in alpha iron, and *vice versa*, those having a body-centered lattice should dissolve more readily in alpha iron. It is also probably true that the more soluble the added element or compound, the less the resulting increase of hardness. Nickel, copper, gold, silver, and many other elements having, like gamma iron, a face-centered cubic space lattice should be more soluble in gamma iron than in alpha iron, but their solution in alpha iron should produce greater hardness. It should be possible by suitably regulated cooling to cause in alloys of iron with one of these metals the formation of some alpha phase, with its very limited solubility for the dissovled metal, along the cleavage planes, giving rise to martensitic structures and therefore to a considerable increase of hardness.

Indeed, the structure known as Widmanstättian is so called

because it was first discovered in meteorites by the French scientist, Alois de Widmanstätten. It is well known that in these alloys of iron and nickel, structures are observed which are singularly suggestive of the structures found in carbon alloys, namely, after slow cooling a pearlitic constituent named "plessite," which itself is a fine eutectoid aggregate of two solid solutions of nickel in alpha iron, "kamacite" and "taenite," the former containing up to 7 per cent. of nickel and the latter richer in that element. These structures result from the breaking up on cooling of a solid solution of nickel in gamma iron.

FIG. 6.—Widmanstättian structure in an alloy of iron and 12% nickel after slow cooling to room temperature. (Carl Benedicks.)

It will be apparent that kamacite and taenite resemble in their behavior the ferrite and cementite of iron-carbon alloys, and that we might refer to plessite as nickel pearlite and to the solid solution of nickel in gamma iron as nickel austenite.

Benedicks prepared an alloy containing about 12 per cent. nickel. After cooling it very slowly to room temperature he obtained the Widmanstättian structure shown in Fig. 6, kamacite having, like ferrite in iron-carbon alloys, separated along the crystallographic planes of the original gamma grains.

The crystal structure of manganese has not been positively determined. Its ready solubility in gamma iron would lead us to expect that it has a face-centered cubic lattice. Con-

trary to the general belief, alloys of iron and manganese do not form homogeneous solid solutions at room temperature. Some recent investigation by Dr. C. R. Wohrman, soon to be published, clearly shows that like iron-carbon alloys and like iron-nickel alloys, a solid solution containing about 3 per cent. of manganese in gamma iron, which we may call manganese

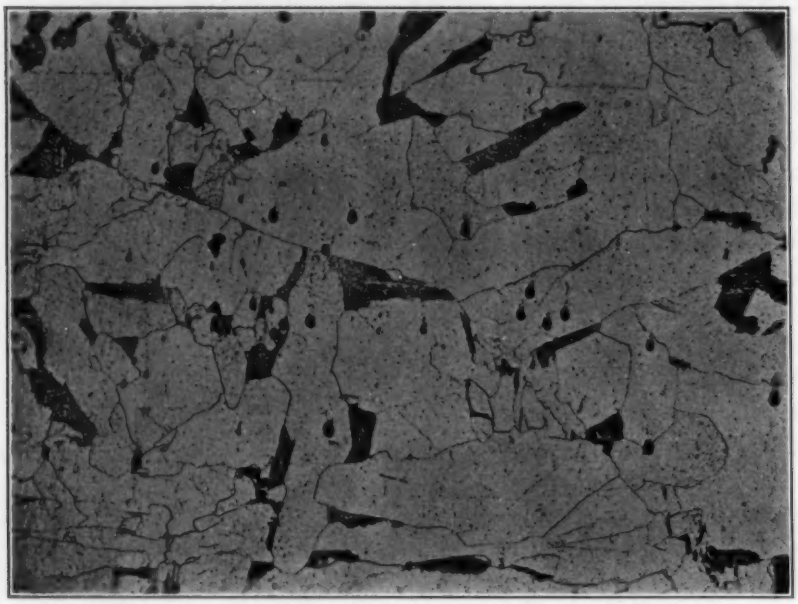


FIG. 7.—Ferrite-Pearlite type of structure in an alloy of iron with 3% manganese in the cast condition and after slow cooling to room temperature. Magnified 100 diameters. (C. R. Wohrman.)

austenite, existing at a high temperature, breaks up on slow cooling into an aggregate of alpha iron which may retain some manganese in solution, and of a pearlitic constituent (manganese pearlite), the exact nature of whose components is still to be determined (Fig. 7). On more rapid cooling, some alpha phase separates at the crystallographic planes, resulting in a martensitic structure and in a considerable increase of hardness (Fig. 8).[1] It is highly probable that this alpha phase

[1] The slowly cooled alloy has a Brinell hardness of 115 while the quenched alloy has a hardness of 265.

retains in solution more manganese than it can normally hold at room temperature. It is therefore supersaturated and of greater hardness. The gamma phase, on the contrary, now richer in manganese than it was originally, may be considered as a solution of manganese in supercooled gamma iron. This analogy between alloys of iron with nickel or with manganese and alloys of iron with carbon is too striking to be lightly dismissed. It is true that carbon martensite, when it contains a sufficient amount of carbon, is considerably harder than nickel

FIG. 8.—Martensitic type of structure in an alloy of iron with 3% manganese after rapid cooling to room temperature, magnified 500 diameters. (C. R. Wohrman.)

or manganese martensite, but this may be due: (1) to the lower solubility of carbon in the gamma phase compared to the solubility of nickel or manganese, seeing that gamma iron cannot dissolve more than 1.70 per cent. carbon; and (2) to the lower solubility of carbon in the alpha phase, a solubility which is practically nil at room temperature. Again, the ready formation of minute particles of cementite in carbon martensite may increase its hardness considerably. The

limited solubility of carbon in both gamma and alpha iron compared to the solubility of nickel and manganese may in turn be explained by the greater dissimilarity between the space lattices of iron and carbon, the latter, in the form of graphite at least, having an hexagonal-close packed lattice.

The factors chiefly influencing the structure and properties of iron-carbon alloys, in accordance with their rate of cooling, may be summarized as follows: (1) the limited solubility of carbon in gamma iron; (2) the nearly complete insolubility of carbon in alpha iron; (3) the formation of a Widmanstättian structure on slow cooling, in which the gamma phase is absent; (4) the formation of a martensitic structure on rapid cooling, in which both the alpha phase and the gamma phase are present; (5) the undercooled condition of the gamma iron present in martensite; (6) the supersaturated condition of the alpha phase present in martensite; (7) the tendency of minute particles of cementite to separate from the alpha phase on aging or on slight reheating (tempering); (8) the tendency of additional gamma iron transforming into alpha iron on aging or on slight reheating (tempering).

Seeing that martensitic steel apparently contains three phases, namely, gamma solid solution, alpha solid solution, and the carbide, Fe_3C, it is not according to the phase rule in a state of stable equilibrium. On heating, therefore, which permits a closer approach at least to a stable equilibrium, one of the phases should be eliminated — undoubtedly the gamma phase, through the transformation of supercooled gamma iron into alpha iron. This may be brought about through a suffi-

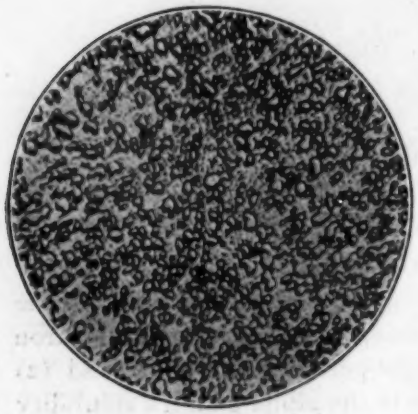


FIG. 9.—Spheroidized structure.

ciently long heating of martensitic steel at about 400° C. The alloy consists then wholly of the alpha phase holding carbon in solution (troostite), and of minute cementite particles. This alpha solid solution, as explained, is supersaturated and tends therefore to reject more of the carbide. Long exposure at some 700° C. permits this rejection to be complete, when the structure shown in Fig. 9 is obtained. The alloy now consists of numerous particles of cementite embedded in alpha ferrite. The heat treatment required to produce this structure is known as spheroidizing. This spheroidized alloy, like the pearlitic alloy resulting from slow cooling (Fig. 10) contains the same proportions of alpha iron and of cementite, but their physical properties differ in a marked degree. "Lamellar" pearlite generally has a Brinell hardness of 200, a tensile strength of 115,000 lbs. per sq. in., an elongation of 10 per cent. in 2 in. The hardness of spheroidized pearlite, on the contrary, may not exceed 125 and its tensile strength 75,000 lbs., while its elongation generally exceeds 15 per cent. It illustrates the marked influence of the character of the structure and of its degree of fineness on the physical properties of alloys of identical composition.

FIG. 10.—Pearlite.

It is surprising that martensite has been considered for so many years to be a solid solution, notwithstanding the many evidences that it is an aggregate. Indeed, there are some who to-day still refer to it in that light.

The views of Zay Jeffries so ably presented and so vigorously defended demand special consideration as they have been accepted, the writer believes, by many. The descriptive title selected by him for his theory, namely, "slip interference," has no special significance because, obviously, increased hardness necessarily implies increased slip interference, quite regardless of the theory one may offer to explain the increase. Jeffries does not consider the presence of both gamma and alpha phases in hardened steel, but attributes its great hardness primarily to the very fine grain size of the alpha iron, a size which he describes as sub-microscopic, and, secondarily, to the presence of many very small particles of cementite. As an evidence of the existence of extremely small grains of alpha iron he points to the blurred appearance of the alpha lines in the X-ray spectrum of hardened steel. Benedicks and others, however, believe that this lack of distinctness may be accounted for as satisfactorily by the distortion of the space lattices of alpha iron resulting from the presence of carbon atoms.

The views briefly expressed in this paper appear to the writer to offer the simplest and most logical explanation of the hardening of steel by rapid cooling and the one best supported by experimental evidence. It is not irreconcilable with Jeffries' theory, for it also holds that the hardness of quenched steel, that is, the hardness of martensite, is due in a large measure to the fineness of its structure.

RECENT LIGHT ON CLASSIC PROBLEMS IN THE THEORY OF NUMBERS

By L. E. DICKSON

(*Read April 29, 1927*)

SIMULTANEOUS with the founding of this Society, Leonard Euler, the greatest mathematician of his time, began his life-long attempt to prove the famous theorem that every positive integer is a sum of four integral squares. It was recently found that there are exactly fifty-four such theorems in which the squares are multiplied by constants. Their proof rests on a knowledge of the form of the numbers represented by a few functions of the type $ax^2 + by^2 + cz^2$; the necessary facts are fully proved in two of my papers now being published. In a third current paper I give a method of finding all quadratic functions of four variables which involve their cross-products as well as their squares and which represent all positive integers.

Three centuries ago, Fermat stated that every positive integer is a sum of three triangular numbers $\frac{1}{2}x(x+1)$ and, more generally, is a sum of n polygonal numbers $\frac{1}{2}(n-2) \times (x^2 - x) + x$ of n sides. In 1815 Cauchy gave the first proof and found that all but four of the polygonal numbers may be taken to be 0 or 1. Here the limitation of x to positive integral values is due to the geometric origin. But simpler results hold if we employ generalized polygonal numbers obtained by using all positive and negative integral values of x. I have just found the improved theorem that every positive integer p is a sum of three generalized polygonal numbers of n sides ($n > 4$) and r numbers 0 or 1, where $r = 0$ if $n = 5$ or 6, $r = 1$ if $n = 7$, $r = 2$ if $n = 8$, and $r = n - 7$ if $n \geqq 9$. For example, every p is a sum of three generalized pentagonal numbers; also a sum of three generalized hexagonal numbers.

In 1770 Waring conjectured that every p is a sum C_9 of nine integral cubes $\geqq 0$, also a sum of nineteen biquadrates (fourth powers), etc. I am publishing two further papers in which I prove the following results. Every p is represented by each of

$$rx^3 + C_8, \qquad sx^3 + 2y^3 + C_7, \qquad tx^3 + 3y^3 + C_7,$$
$$r \leqq 23, \qquad s \leqq 34, \qquad t \leqq 9;$$

but the proof is not yet complete if s or t is divisible by 5. Every p is a sum of k doubles of biquadrates and $37 - 2k$ biquadrates. Every p is represented by each of

$$S_{118} + 8S_{108}, \qquad S_{10} + 2S_{90} + 3S_{72} + 4S_{18},$$
$$E_{1786} + 6E_{456} + 60E_{448},$$

where S_n is a sum of n sixth powers, and E_n is a sum of n eighth powers. Each of these forms involves fewer variables than the number of powers in the corresponding Waring theorem as proved to date.

Finally, I am publishing three more papers in which I give extensive verification of my remarkable empirical theorems generalizing that of Waring by affirming that certain of the nth powers may be taken equal, then a further set of them equal, etc. Here again we have a material reduction in the necessary number of variables.

If we replace x by $-t$ in the above definition of ordinary polygonal numbers, we obtain the *extended* polygonal number $e(t) = \frac{1}{2}m(t^2 + t) - t$ of $m + 2$ sides, where t is an integer $\geqq 0$. If $m = 2$, $e(t) = t^2$ is a square. If $m = 1$, $e(t)$ is the triangular number $\frac{1}{2}(t - 1)t$, which is zero when $t = 0$ or 1. Hence if $m = 1$ or 2, every integer is a sum of 3 or 4 numbers $e(t)$ by known theorems quoted above.

We shall prove the following new result:

THEOREM. *If $m > 2$, every positive integer N is a sum of four extended polygonal numbers of $m + 2$ sides and $m - 2$ numbers 0 or 1.*

This means that there exist integers t, u, v, w, r, each $\geqq 0$, such that

$$(1) \quad N = e(t) + e(u) + e(v) + e(w) + r, \qquad r \leqq m - 2.$$

If we employ the abbreviations

$$k = t^2 + u^2 + v^2 + w^2, \qquad s = t + u + v + w, \tag{2}$$

we see that (1) is equivalent to

$$N = \tfrac{1}{2}m(k + s) - s + r. \tag{3}$$

LEMMA. *If k is odd and if s is an odd integer lying between $K = \sqrt{3k - 2} - 1$ and $\rho = \sqrt{4k}$, equations (2) have integral solutions t, u, v, w each $\geqq 0$.*

Then $a = 4k - s^2$ is positive and clearly has the remainder 3 when divided by 8. A classic theorem shows that a is a sum of three integral squares

$$a = x^2 + y^2 + z^2. \tag{4}$$

Evidently x, y, z are all odd; they may be assumed to be positive. The identity

$$(x + y + z)^2 + (y - z)^2 + (z - x)^2 + (x - y)^2 = 3(x^2 + y^2 + z^2)$$

gives

$$x + y + z \leqq \sqrt{3a}.$$

Also, $K < s$ implies $3k < s^2 + 2s + 3$. Hence

$$3a = 3(4k - s^2) < 4(s^2 + 2s + 3) - 3s^2,$$
$$x + y + z < \sqrt{s^2 + 8s + 12} < s + 4.$$

Write $t = \frac{1}{4}(s - x - y - z)$. Then $-t < 1$. If t is an integer, it is $\geqq 0$, and

$$u = \tfrac{1}{4}(s - x + y + z), \qquad v = \tfrac{1}{4}(s + x - y + z),$$
$$w = \tfrac{1}{4}(s + x + y - z)$$

are all integers $\geqq 0$, since $u - t = \frac{1}{2}(y + z)$ is an integer, etc. Then

$$t + u + v + w = s,$$
$$t^2 + u^2 + v^2 + w^2 = \tfrac{1}{4}(s^2 + x^2 + y^2 + z^2) = k,$$

and hence (2) are satisfied. But if t is not an integer, $t_1 = \frac{1}{4}(s + x + y + z)$ is an integer, since $t_1 - t = \frac{1}{2}(x + y + z)$ and t are both halves of odd integers. Then

$$u_1 = \tfrac{1}{4}(s + x - y - z), \qquad v_1 = \tfrac{1}{4}(s - x + y - z),$$
$$w_1 = \tfrac{1}{4}(s - x - y + z)$$

are all integers $\geqq 0$; they with t_1 satisfy (2).

For each odd k, there exists an odd s lying between K and ρ. For $k > 9$, we have $\rho - K > 2$, whence there are at least two consecutive integers between those limits. According as $k = 1, 3, 5, 7, 9$, we have $s = 1, 3, 3, 5, 5$.

Let l be the least and g the greatest odd integer between K and ρ, with $l = g$ if there be only one such integer. Let A_k denote the function (3). Its least value for $r = 0, 1, \cdots, m - 2$ and $s = l, l + 2, l + 4, \cdots, g$ is

$$L_k = \tfrac{1}{2}m(k + l) - l, \tag{5}$$

since $m > 2$, and its greatest value is

$$G_k = \tfrac{1}{2}m(k + g) - g + m - 2. \tag{6}$$

The various values of A_k are

$$\begin{aligned}
&L_k, 1 + L_k, 2 + L_k, \cdots, m - 2 + L_k,\\
M = \tfrac{1}{2}m(k + l + 2) - l - 2,\ &1 + M, \cdots, m - 2 + M,\\
&\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\\
P = \tfrac{1}{2}m(k + g - 2) - g + 2,\ &1 + P, \cdots, m - 2 + P,\\
Q = \tfrac{1}{2}m(k + g) - g,\ &1 + Q, \cdots, m - 2 + Q = G_k.
\end{aligned}$$

The last number of any row is seen to be equal to the first number of the next row. Hence the values of A_k coincide with all the integers from L_k to G_k inclusive. By the lemma and (1) – (3), we conclude that the theorem holds for all these integers.

This result holds for every odd k and hence also for $k + 2$. Let l' be the least and g' the greatest odd integer between

$$K' = \sqrt{3(k + 2) - 2} - 1 \quad \text{and} \quad R = \sqrt{4(k + 2)}. \tag{7}$$

Hence the theorem holds for all integers from

$$L_{k+2} = \tfrac{1}{2}m(k + 2 + l') - l' \tag{8}$$

to

$$G_{k+2} = \tfrac{1}{2}m(k + 2 + g') - g' + m - 2, \tag{9}$$

inclusive. Both $K' - K$ and $R - \rho$ are less than 2. Hence

l' is l or $l+2$, while g' is g or $g+2$. By (6) and (9),

$$G_{k+2} - G_k = m + (\tfrac{1}{2}m - 1)(g' - g) \geqq m \geqq 2.$$

Hence G_k, G_{k+2}, G_{k+4}, $\cdots$ increase indefinitely.

Since l' is one of the first two terms of the series l, $l+2$, $\cdots$, g, we have $l' \leqq g$. First, let $l' < g$. Then $l' \leqq g-2$. Write $l' = g - 2 - p$, $p \geqq 0$. By (8) and (6),

$$L_{k+2} = \tfrac{1}{2}m(k+g-p) - g + 2 + p$$
$$= G_k + 4 - m + p(1 - \tfrac{1}{2}m).$$
$$= G_k + 1 - d, \qquad d = m - 3 + p(\tfrac{1}{2}m - 1) \geqq 0,$$
$$L_{k+2} \leqq 1 + G_k.$$

Our theorem was proved for all integers from L_{k+2} to G_{k+2}, inclusive, and for G_k. Hence it holds for all integers from G_k to G_{k+2}, inclusive.

Second, let $l' = g$. The theorem holds for all integers from $L_{k+2} = 2 + G_k$ to G_{k+2}, inclusive, and for G_k. It will be verified presently for $1 + G_k$. It will then hold for all integers from G_k to G_{k+2}, inclusive. This was proved also in the first case. Since these G's increase indefinitely, the theorem holds for all integers $\geqq G_k$. It was seen to hold from L_k to G_k. Hence the theorem holds for all integers $\geqq L_k$. We may take $k = 1$. Then $l = 1$, $L_1 = m - 1$. Since the theorem is evident for integers $\leqq m - 2$, it holds for all integers.

It remains only to prove the theorem for $1 + G_k$ when $l' = g$. By definition, l' is the least odd integer $> K'$ in (7), while g is the greatest odd integer $< \rho = \sqrt{4k}$. But if $\rho - K' > 4$, there are four consecutive integers between K' and ρ, two of which are odd, contrary to $l' = g$. Hence $\rho - K' \leqq 4$, which implies $k < 134$. The odd integers $k < 134$ for which $l' = g$ are shown in the following table. The sets of values in the left column satisfy

$$(10) \quad k + 1 = t^2 + u^2 + v^2 + w^2, \qquad g - 1 = t + u + v + w.$$

The sets in the right column satisfy

$$(11) \quad k + 1 = t^2 + u^2 + v^2 + w^2, \qquad g + 1 = t + u + v + w.$$

k	g	t	u	v	w
109	19	7	6	5	0
107	19	7	7	3	1
89	17	8	4	3	1
87	17	8	4	2	2
85	17	8	3	3	2
65	15	5	5	4	0
55	13	6	4	2	0
53	13	5	5	2	0
47	13	4	4	4	0
37	11	5	3	2	0
35	11	4	4	2	0
33	11	4	4	1	1
23	9	4	2	2	0
21	9	3	3	2	0
17	7	3	3	0	0
13	7	3	2	1	0
9	5	3	1	0	0
7	5	2	2	0	0
3	3	2	0	0	0

k	g	t	u	v	w
71	15	6	4	4	2
69	15	5	5	4	2
67	15	5	5	3	3
51	13	4	4	4	2
49	13	4	4	3	3
41	11	4	4	3	1
39	11	4	4	2	2
29	9	4	3	2	1
27	9	3	3	3	1
25	9	3	3	2	2
15	7	2	2	2	2

From (6) and (10), we get

$$1 + G_k = \tfrac{1}{2}m\sum(t^2 + t) - \sum t + m - 2.$$

From (6) and (11), we get

$$1 + G_k = \tfrac{1}{2}m\sum(t^2 + t) - \sum t.$$

In the respective cases, $1 + G_k$ is a sum of four values of $e(t)$ and either $m - 2$ or 0. This proves the theorem for $1 + G_k$ and is the last step in the complete proof of the theorem.

VALENCE

By WILLIAM ALBERT NOYES

(*Read April 29, 1927*)

Some persons who have little knowledge of science and who wish to discredit its achievements, take pleasure in saying that accepted theories are often discarded and replaced by others. A more careful study of the progress of scientific knowledge reveals the fact that the earlier theories corresponded, in some measure, with the realities of nature. A fuller knowledge has made it possible to correct and amplify the crude and imperfect ideas of the earlier time, and we have reason to believe that our pictures approach more closely to those realities we seek to know.

Thus, the Ptolemaic System gave a much less perfect picture than the Copernican, of the actual motions of the planets and stars, but, in spite of that, it was in close agreement with the things we see and its epicycles enabled the older astronomers to predict eclipses.

When many observations of phenomena, which seem quite unrelated, are consistent with a single, comparatively simple explanation, we gain confidence that our inquiries are proceeding in a fruitful direction and are approaching the truth.

The history of the theory of valence furnishes a particularly suitable illustration of these observations. During more than two centuries, the explanations of electrical, physical and chemical phenomena have grown from very simple beginnings to the complex and still incomplete theories of to-day. I wish to sketch the development of these theories. It is particularly interesting to note that, quite often, ideas of an early time have been abandoned as useless, only to reappear in a modified form as a more comprehensive knowledge of experimental facts shows that they were partly true.

That amber, when rubbed, will attract and then repel light bodies, was known to the Greeks, but conductors of electricity were first recognized by Gilbert, and vitreous and resinous electricity were distinguished by Dufay only two hundred years ago. In recent times we have come back to the idea that these two kinds are inherent in the very constitution of matter, but about 1750 Benjamin Franklin introduced the idea that there is only one kind of electricity and that a conductor connected with electrified glass contains an excess, and one connected with electrified sulfur a deficiency, of the "fluid." If we put electrons in place of "fluid" and correct his mistake about the direction in which the electrons move, his ideas and ours are in close accord. Perhaps some day our physicists and electrical engineers will have the courage to correct an error which causes much confusion.

Lavoisier gave the first comprehensive theory of chemical compounds in a dualistic system having oxygen as the central element. To him, acids were compounds of non-metals with oxygen, and bases, compounds of oxygen with metals. Salts were also dualistic compounds of a second order.

Sulfur + Oxygen = Sulfuric Acid,
Calcium + Oxygen = Lime,
Sulfuric Acid + Lime = Sulfate of Lime.

In 1786, Galvani discovered current electricity and soon after Volta invented the first primary batteries. Early in the nineteenth century, Davy used the new batteries for the decomposition of caustic potash and caustic soda and demonstrated the intimate connection between electrical and chemical phenomena. About the same time, Dalton gave the world the atomic theory and Davy and Berzelius soon translated Lavoisier's dualistic system into the old electrochemical theory, in which oxygen was negative toward a metal and positive toward a non-metal.

$$\overset{+}{\mathrm{Ca}}\overset{-}{\mathrm{O}} + \overset{-}{\mathrm{S}}\overset{+}{\mathrm{O}}_3 = \overline{\overset{+}{\mathrm{Ca}}\overset{-}{\mathrm{O}}}\cdot\overline{\overset{-}{\mathrm{S}}\overset{+}{\mathrm{O}}_3}.$$

In 1834, Faraday discovered that the same electrical

current passed through a series of electrolytes causes the separation of different elements in quantities proportional to their chemical equivalents.

Ag	Cu	H	Sn
108	31.8	1	59.3

Faraday was very cautious about drawing any far-reaching theoretical conclusions from his experiments and it was nearly fifty years later before Helmholtz, in his Faraday lecture before the London Chemical Society, stated the logical inference that each chemical atom must have associated with it an atom of electricity. Even Helmholtz must have had only a very vague notion of the meaning of "atom" as applied to electricity.

In 1840, Dumas made a discovery which seems to be a step backward and which completely discredited the older electrochemical theory. He found that three of the hydrogen atoms of acetic acid could be replaced by chlorine and that the trichloroacetic acid was still a monobasic acid with decompositions closely analogous to those of acetic acid, giving chloroform, for instance, in place of methane. Hydrogen was positive and chlorine negative under the old system and it seemed impossible that one should take the place of the other.

A theory which has secured very general acceptance dies hard, and Dumas's substitution theory was greeted by a clever letter, written in French by Wöhler, but signed S. C. H. Windler. It purported to come from Paris and gave an account of experiments in which manganese acetate was treated with chlorine. The hydrogen was replaced first, then the oxygen, the manganese and, finally, the carbon. It was claimed that the compound still had some of the properties of manganese acetate, though the author admitted that it looked like chlorine hydrate. Wöhler had studied with Berzelius and was very loyal. The article illustrates Berzelius's attitude

and that of his followers toward the new ideas. But Berzelius came into sharper and sharper conflict with the facts of organic chemistry and the older electrochemical theory slowly disappeared from serious consideration. Negative and positive groups and elements were often spoken of during the last half of the nineteenth century, but it is difficult to attach any definite *electrical* meaning to the words.

In 1852, Frankland laid the first definite foundation for the theory of valence. He began with the valences of tin and arsenic but extended the principle to other elements. He used the old system, with eight for the atomic weight of oxygen, recognizing the trivalence of nitrogen in nitrous anhydride and ammonia, and its quinquivalence in ammonium iodide and nitric anhydride, more easily, because he wrote the formulæ:

$$NO_3,\ NH_3,\ NO_5,\ NH_4I.$$

In 1858, Couper and Kekulé, quite independently, extended the idea of valence to include chains of carbon atoms. Couper's paper was written first and the failure of an editor to give it prompt publication has robbed him of credit which he surely deserves.

The new theory quickly replaced the type-formulæ previously in vogue and for forty years nearly all the chemists in the world devoted themselves to deciphering the structure of known organic compounds and to building new ones predicted by the theory. During this period the atoms were pictured as holding to each other in definite sequences but the nature of the force which binds them together was rarely considered.

The basis for our modern electrochemistry came along quite different lines. Hittorf discovered, in 1859, that the transference of ions through a solution of an electrolyte occurs at different rates for different ions but he did not see that this points very clearly to the independence of the ions in a solution.

In 1878, Crookes discovered the cathode rays, which are streams of electrons shot out perpendicularly from the surface of the cathode. He could not see the full meaning of this

remarkable discovery but he demonstrated the electrical character of the rays by deflecting them with a magnet and their material character by making them impinge on the vanes of a little wheel placed in their path, causing it to rotate.

In Crookes's discovery lay the beginning from which has come X-rays, the better knowledge of electrons, a knowledge of radio-activity, some principles used in radio-telegraphy, and a deeper insight into the structure of atoms and molecules. It would be difficult to name any other discovery since Dalton's discovery of atoms, which has been as important.

In 1887, Arrhenius proposed his theory of the ionization of electrolytes. This gave a new concept of positively and negatively charged particles as independent molecules in a solution, having the kinetic and diffusive properties of the molecules of a gas.

Röntgen rays were discovered, almost by accident, in 1895. We now think of atoms as consisting of positive nuclei with electrons rotating about them in successive shells. For all atoms except hydrogen, two of these electrons are in very rapid rotation close to the nucleus. These electrons and others are supposed to change their orbits, moving away from the nucleus as they receive energy of a definite wave length from an external source, and giving out energy of the same wave length as they fall from one orbit to another closer to the nucleus. These waves of light characteristic of the two inner electrons, give the two X-ray spectral lines of Moseley, which have been the occasion for the discovery of several new elements, notably hafnium and illinium, and have made it possible to fix positively the gaps in the Periodic System which remain to be filled.

In 1897, Thomson in England and Kaufmann in Germany recognized more clearly the material nature of the cathode rays and Thomson, by measuring the acceleration of electrons in an electrical and a magnetic field and the heat energy produced when the electrons fall on a thermo-pile, found the ratio between the mass and the electrical charge and demonstrated that the mass is 1800 times less than that of a hydrogen atom.

It is well to remember that the mass of the electron has been determined by means of the accelerations produced by electrical forces, or, in the experiments of R. C. Tolman, by means of the electrical forces produced when electrons are stopped. Whether the mass measured by the acceleration produced by gravitational force would be the same, is a question of very considerable interest, but one which, so far, remains unanswered. Since an electron with a velocity of 10,000 kilometers a second would fall only about eight millimeters in flying from here to Boston, the difficulties of the measurement are evident.

The discovery that uranium may produce effects similar to those of X-rays, was made by Becquerel in 1898. This was soon followed by the discovery of radium and radio-active elements by the Curies. In 1902–03, Rutherford proposed the theory of the disintegration of atoms, which has helped so much toward a knowledge of atomic structure.

Here, again, I wish to anticipate later discoveries because of the bearing which radioactive disintegrations have on some phases of the theory of valence. I have spoken of the belief that each atom consists of a central positive nucleus with electrons rotating about it. We have reason to think that each element in the Periodic System differs from the one before it by having one more unit charge in its positive nucleus and one more electron rotating about that nucleus. Two kinds of radioactive disintegration occur.

In one, the atom expels from its center a helium nucleus carrying a positive charge of two units. When this happens, the atom immediately loses two electrons of the outer, or valence, layer and there remains an atom two steps down in the System. Thus, radium disintegrates by expelling a helium nucleus (not a helium atom as it is sometimes carelessly stated) from its center, and then two valence electrons. The atom which remains is radon, one of the noble gases with no valence electrons.

In the other sort of disintegration, an atom expels an electron from its center. This causes an increase of one unit

in the positive charge of its nucleus. The residue immediately picks up an additional valence electron and the new atom is one step up in the Periodic System.

The point which I wish to emphasize is, that an exact balance between the number of unit positive charges in the nucleus and the number of electrons surrounding it is a fundamental characteristic of each electrically neutral atom. It must also be a characteristic of each independent molecule.

The new electrochemical theory of Arrhenius was used, at first, only for the discussion of reactions between electrolytes. In 1895, Vant Hoff, for the first time, suggested an ionic explanation for a reaction in which ordinary electrolytes were not involved—the formation of ozone in the slow oxidation of moist phosphorus. He never followed the idea further.

Perhaps I may be pardoned for relating a personal incident which occasioned my interest in the subject. In the late nineties, I gave my lectures in a room separated from my laboratory. The bottle of ammonia stood for some years in this room and became dilute. One day, in trying the old Hofmann experiment, in which ammonia is introduced into a tube filled with chlorine and followed by acid and water, leaving the tube one third full of nitrogen, it occurred to me that the acid was not necessary, as the water would absorb the excess of ammonia. I tried the experiment in that way before my class without trying it out beforehand, as every good lecturer should. To my discomfiture, after admitting the water, the tube was only one sixth full of nitrogen. I passed the matter off as well as I could and tucked the experience away in my memory as something that would bear looking into. I was busy with camphor in those days, but a few years later I put Mr. Lyon, one of our seniors, on the problem and we found that the deficiency in nitrogen was due to the formation of nitrogen trichloride.

In explanation of the reaction, I suggested that the chlorine of the nitrogen trichloride is positive and that the chlorine molecules separated into positive and negative atoms in the reaction. Professor Stieglitz almost immediately published a

note on positive and negative halogen ions in which he stated that he had presented similar views before his students. It was rather the accident that we had happened on a reaction requiring an explanation of this kind, than anything else that had led to our priority in publication. Lapworth, in England, published a paper the same year, in which he supposed bromine to separate into positive and negative ions. A criticism by A. A. Noyes led me, in 1904, to assume that ions are formed during chemical reactions rather than that they already exist in the molecules which separate into positive and negative parts. This is very close to my present view.

A good many years later Professor Bray pointed out that the ninth order reaction assumed in our paper is highly improbable and some of the other suppositions of that paper are no longer tenable. Nevertheless, the idea that compounds which are not electrolytes often separate into positive and negative parts during reactions seems destined to find a permanent place in our theories.

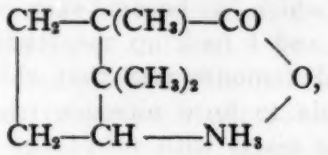
In the hypothesis proposed in 1901, no reference was made to electrons. In 1904, J. J. Thomson suggested that atoms may consist of spheres of positive electricity with electrons inside, and that the static forces developed by the transfer of electrons from one atom to another might account for chemical combination. The work of Rutherford and his colleagues has demonstrated that the positive charge of an atom is concentrated in a small nucleus and Thomson's hypothesis in its original form has been abandoned. However, Falk and Nelson, Fry, Stieglitz, L. W. Jones and others used Thomson's idea of the transfer of an electron from one atom to another to explain many reactions of organic compounds.

Following the same idea, I suggested in 1913, that since the nitrogen is negative and the chlorine positive in ordinary nitrogen trichloride, there should be another trichloride in which the nitrogen is positive and the chlorine, negative. For more than ten years I sought for such a compound with the aid of several able assistants. Our failure is one of the reasons why I now think that the polar character of atoms joined

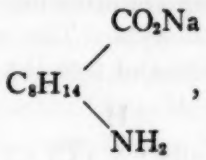
by a covalence, as in nitrogen trichloride, is potential rather than complete. I will return to this later.

In 1913, Bohr proposed his hypothesis that an atom consists of a central positive nucleus with electrons rotating about it, and that the same electron may revolve in orbits at different levels and emit or absorb light in "quanta" as it passes from one level to another. Bohr, Sommerfeld and others have been very successful in explaining spectral lines on the basis of this theory and it seems probable that it is a close approximation to the reality, though some parts of the theory are far from clear.

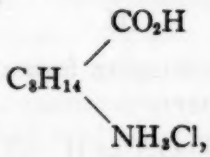
In 1915, Mr. Potter and I published a series of observations on the optical rotations of aminocamphonanic acid,



and related compounds, which led us to the conclusion that the fifth valence of the nitrogen atom holds it to the oxygen in a stable configuration, giving to the aminocamphonanic acid a left-handed rotation of polarized light, while the sodium salt,



and the hydrochloride,



rotate to the right. This has become of more than usual interest because the valence between the nitrogen (or the ammonium group) and the oxygen is "polar" and G. N. Lewis and others have wished to exclude such relations from the category, "valence."

In 1916, Professor G. N. Lewis proposed his theory that atoms have a strong tendency to assume a form in which there are eight electrons in their external shells, either by dropping or taking up electrons to form ions or by sharing pairs of electrons with other atoms. Professor Lewis conceived the idea at the basis of his theory in 1902 and I think we may well compare the years during which he thought it over before publishing it with the years of incubation for the theory of evolution in the mind of Darwin.

In 1919, Langmuir gave a brilliant public exposition of the theory and contributed greatly to the attention which it has received. Langmuir introduced the term "covalence" to designate the bond between atoms held together by a pair of electrons—a term which has become very convenient.

In 1920, Haw and I took up the study of nitrogen trichloride again and demonstrated that chlorine reacts with anhydrous ammonia to form nitrogen trichloride and that nitrogen trichloride reacts with anhydrous hydrochloric acid to form ammonium chloride and chlorine. These reactions are most simply explained by assuming that a positive chlorine atom adds to ammonia to form the chloroammonium ion, NH_3Cl, and that a hydrogen atom of this ion splits off with the chloride ion, leaving chloroamine, NH_2Cl. Two repetitions give nitrogen trichloride. The reverse of this reaction gives ammonium chloride and free chlorine.

$$\begin{matrix} & H & & & H & & & Cl & \\ H:&\ddot{N}&:+\,Cl:Cl \rightleftarrows H:&\ddot{N}&:Cl.Cl \rightleftarrows H:&\ddot{N}&:+\,HCl. \\ & H & & & H & & & H & \end{matrix}$$

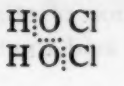
In 1901, Professor Stieglitz formulated the reaction between chlorine and water:

$$Cl—Cl + H—OH \rightleftarrows H—Cl + Cl—OH.$$

If this formulation is correct, hypochlorous acid should be amphoteric and ionize both as an acid and as a base. Wilson and I demonstrated that this is almost certainly true, in 1922.

Previous investigators had failed to get the true conduc-

tivity of solutions of hypochlorous acid because of the rapidity with which decomposition products are formed, even under the influence of an alternating current. Mr. Wilson avoided this difficulty by causing the solution to flow through the cell while the measurements were made. In this way he obtained the constant, $k = 6.70 \times 10^{-10}$, for hypochlorous acid and, by determining, also, the conductivity of a solution of hypochlorous acid to which sodium hydroxide was added, he showed that the conductivity was due to hydrogen ionization. The addition of dilute nitric acid to a solution of hypochlorous acid failed to show any measurable hydroxyl ionization. We were able, however, to show that some hydroxyl ionization takes place by use of a suggestion from a paper published by Balard in 1834. He states that hypochlorous acid is formed when a solution of the acid is treated with anhydrous calcium nitrate. The compound which Balard called "hypochlorous acid" was, of course, in accordance with the nomenclature of that time, chlorine monoxide. We passed a measured volume of air through a solution of hypochlorous acid at 25°, then through a tube containing calcium nitrate and through bulbs containing sodium hydroxide. By noting the gain in weight of the bulbs and titrating the solution it was shown that the gas absorbed was chlorine monoxide, as Balard stated. It was easy to calculate the weight of water vapor the volume of air used took up from the solution. If the chlorine left the solution as hypochlorous acid, the gain in weight of the calcium nitrate would have been greater than this by the weight of the water from the decomposition of the hypochlorous acid. It was found that the gain in weight corresponded to the weight of the water vapor alone. The vapor phase above a solution of hypochlorous acid contains, therefore, chlorine monoxide, Cl_2O, and not hypochlorous acid, HClO. As Jawkowin has shown, by freezing point determinations, that the solution contains, mainly, HClO, these results can be simply explained only by assuming an amphoteric ionization:



In 1922, Mr. Hibben demonstrated that the trimethylethoxyammonium ion, $(CH_3)_3NOC_2\dot{H}_5$, migrates toward the cathode while the hydroxyl ion associated with it migrates toward the anode. We also thought that we had shown that the conductivity of a solution of trimethylethoxyammonium hydroxide is much less than it should be if highly ionized. Stewart and Maeser demonstrated that this conclusion was due to errors of observation and their results were confirmed by more direct experiments in our laboratory.

Both Stewart and Maeser and I found, however, that solutions of trimethylamine oxide, $(CH_3)_3NO$, are only slightly ionized. But trimethylhydroxyammonium bromide, $(CH_3)_3NOH.Br$, formed by adding hydrobromic acid to a solution of the oxide, is highly ionized.

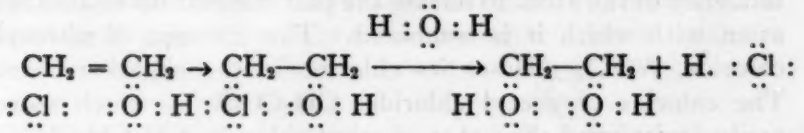
Two important conclusions follow:

1. The solution of trimethylamine oxide can not contain any appreciable amount of the ion, $(CH_3)_3NO\dot{H}$, and the formula, $(CH_3)_3N : O : H : O : H.H_2O$, which Lewis and some others have ascribed to the hydrate of the oxide, is very improbable.

2. The high degree of ionization of quaternary ammonium compounds is due to the fact that the nitrogen atom of these compounds shares four pairs of electrons with other atoms rather than to the nature of the atoms with which the electrons are shared. Trimethylhydroxyammonium bromide is as highly ionized as tetramethylammonium bromide. We approach pretty close, here, to the principle which S. C. H. Windler ridiculed in 1840.

As late as 1923, it was supposed that the positive-negative theory of chemical reactions was incompatible with Lewis's theory of shared electrons. At the Faraday Society Symposium in Cambridge, in 1923, I suggested that if we suppose the pair of shared electrons to remain with one or the other of the reacting atoms, the two points of view may be easily reconciled. This method of interpreting reactions is well illustrated by the formation of ethylene chlorohydrin by the addition of hypochlorous acid to ethylene and the hydrolysis

of the chlorohydrin to ethylene glycol and hydrochloric acid.



It is evident that a chlorine atom which adds to the carbon atom in the positive form, with only six electrons, hydrolyses from it in the negative form, with eight electrons. The chlorine of the hypochlorous acid is potentially positive, but it becomes potentially negative when it has shared a pair of electrons with the carbon. In this form, the positive-negative theory gives us no reason for thinking the two atoms of the chlorine molecule different. The strong tendency of pairs of electrons to remain together causes one of the atoms to become positive and the other negative, *as they react.*

Potential polarities are closely related to a statement frequently made by Lewis and Langmuir that a pair of electrons shared by two atoms is drawn toward the atom with the larger kernel charge. There are three principles which seem to govern potential polarities:

1. When two atoms separate, the pair of electrons tends to go with the atom having the larger kernel charge. In nitration reactions, the electrons remain with the hydroxyl because oxygen has a kernel charge of six and nitrogen one of five.

$$C_6H_5H + HO.NO_2 = C_6H_5NO_2 + HOH.$$

"Kernel" is used here as defined by Lewis, to designate the portion of an atom within the shell of valence electrons. The kernels of oxygen and nitrogen include only the positive nucleus and the two inner electrons.

2. The pair of electrons tends to remain with the atom having its shell of valence electrons closer to the nucleus. In nitrogen trichloride, the nitrogen, with only two inner electrons in its kernel, retains the pair which hold it to the chlorine and the chlorine, having ten electrons in its kernel, separates in the positive form.

3. Union with an atom of high kernel charge enhances the tendency of the atom to release the pair of electrons to another atom with which it is combined. The nitrogen of nitrosyl chloride, NOCl, releases its chlorine in the negative form. The chlorine of acetyl chloride, CH_3COCl, is much more easily hydrolyzed than that of ethyl chloride, CH_3CH_2Cl.

Professor Stieglitz and, if I understand him correctly, Professor Lewis, prefer to call the unions which I designate as "potentially" polar, simply as polar, and the latter would call unions in which the electrons remain entirely with one of the atoms as ionic, while the latter thinks there are no really ionic *unions*. The difference between us is largely one of nomenclature and, in the light of what follows, it seems to me better to call unions in which a pair of electrons is shared by two atoms "potentially" polar and only those unions or temporary conditions in which the pair of electrons remain entirely with one atom, polar. This corresponds quite closely to distinctions drawn by Professor Lewis in 1913.

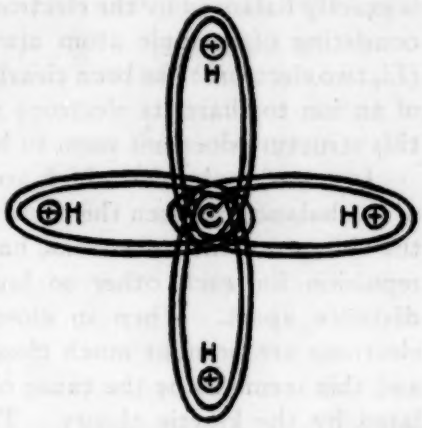
In 1917, I suggested the hypothesis that two atoms may be held together by an electron rotating about two positive nuclei. Misled by the fact that the nucleus of helium maintains its identity in radio-active atoms, I assumed four positive nuclei in a carbon atom and I also assumed a single electron as forming the bond. In both respects my hypothesis was almost certainly wrong.

In 1923, Sidgwick and C. A. Knorr, independently of my suggestion and of each other, proposed a similar hypothesis in a much better form. The figure represents the structure of methane as given by Knorr.

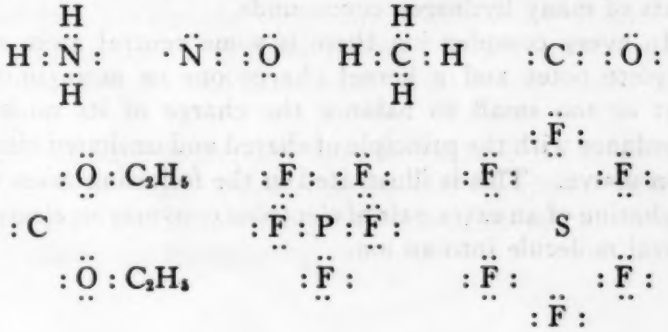
More recently, Paulus and Grimm and Sommerfeld have favored this hypothesis and Glockler has given it some experimental support by showing that the ionization potential of methane is nearly the same as that of argon.

It will be easily seen that this hypothesis supposes a very definite distinction between a covalence, in which the pair of electrons rotate about the kernels of two atoms and a polar valence, in which the pair belong exclusively to the negative atom or ion.

It has been pointed out that the atoms formed in radioactive changes always have an exact balance between the number of units of positive charge in their nuclei and the number of electrons surrounding the nucleus. This principle is so fundamental that in their most stable form at high temperatures such atoms as those of chlorine, bromine and iodine have only seven valence electrons. The principle of balance between the nucleus and the surrounding electrons is more fundamental than the octet.



It has been recognized, of course, that in every molecule the same principle of balance between the sum of the nuclear charges and the number of electrons must exist and that, in general, the pair of electrons of a covalence balances one charge on each of the atoms held together. It has not been quite so clearly seen that this principle applies to *each* atom in any compound containing only covalences. In such compounds, *the sum of the pairs of shared electrons (or covalences) plus the number of unshared electrons must equal the number of valence electrons for each atom.* The following illustrations show that this principle of balance between the charge of the kernel and the numbers of shared and unshared electrons applies in some cases where the principle of the octet fails.

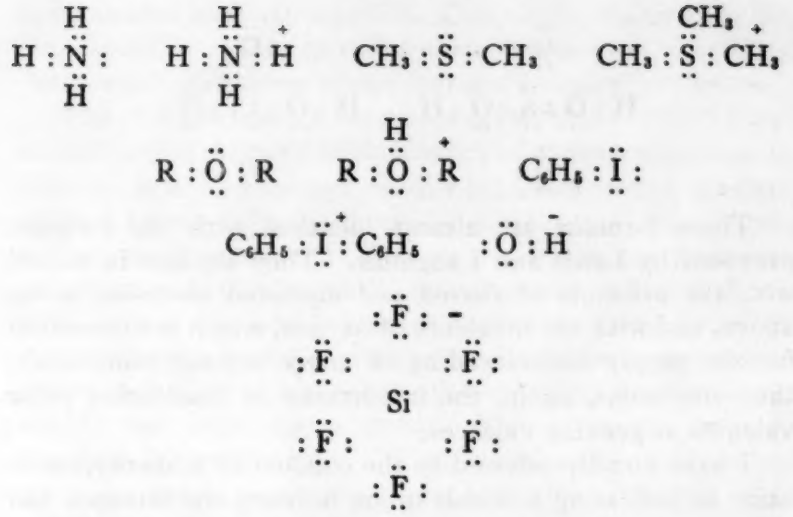


According to the theory of Professor Lewis, atoms of the noble gas series have an octet of electrons on the outside (helium, two electrons) and the positive charge of the nucleus is exactly balanced by the electrons surrounding it. That ions consisting of a single atom always have a completed octet (Li, two electrons) has been clearly recognized but the inability of an ion to share its electrons with other atoms because of this structure does not seem to have been emphasized.

In a gas, molecules which are electrically neutral because of the balance between the positive charges of their nuclei and the charges of their electrons, have no electrical attraction or repulsion for each other so long as they remain at some distance apart. When in close proximity, however, their electrons are brought much closer together than their nuclei and this seems to be the cause of the elastic collisions postulated by the kinetic theory. The fact that these collisions produce no permanent effects on the orbits of the electrons in gases which do not react, is probably connected with the inability of electrons to give out or absorb energy otherwise than in definite "quanta." If we assume that something like the same relation occurs between the ions of a solution, it helps us to understand why these ions maintain their independent existence and obey the gas laws as though they were independent molecules. The dielectric properties of the solvent are, of course, important. The static electric forces between sodium and chlorine atoms hold them together as molecules of sodium chloride in the gaseous state.

The fact that the hydrogen ion has no external electron seems to be closely connected with the low ionization constants of many hydrogen compounds.

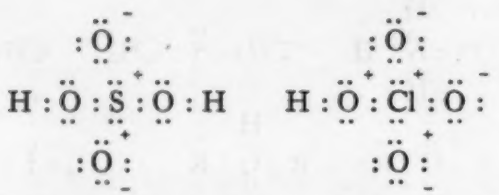
In every complex ion there is some central atom with a complete octet and a kernel charge one or more units too great or too small to balance the charge of its nucleus in accordance with the principle of shared and unshared electrons given above. This is illustrated in the following cases where the sharing of an extra pair of electrons converts an electrically neutral molecule into an ion.



In hydroxyl there is one unshared electron in excess, giving a univalent negative ion. In the hydrofluosilicic ion there are two pairs of shared electrons in excess, giving a bivalent, negative ion. The arrangement in this ion is probably similar to that in sulfur hexafluoride.

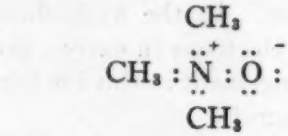
At the symposium of the Faraday Society on the Electronic Theory of Valency, held at Cambridge in 1923 Lowry suggested that double bonds between atoms may sometimes consist of one covalence and one polar valence. He called these "mixed double bonds." Sugden, in 1925, suggested that such bonds should be called "semipolar" and, by means of a function which he calls the "parachor," based on the surface tension and density of compounds, he showed that it is possible to distinguish between semipolar bonds and bonds due to two covalences. The double bonds of unsaturated carbon compounds and those between carbon and oxygen in aldehydes, ketones and acids seem to be of the double covalence type.

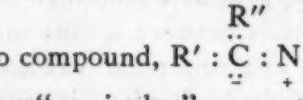
In a personal letter, Dr. Sugden has told me that he has obtained very positive evidence for two semipolar unions in sulfuric acid and three semipolar unions in perchloric acid. This leads to the formulæ:

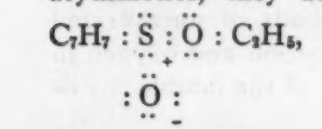


These formulæ are almost identical with the formulæ proposed by Lewis and Langmuir. They are also in accord with the principle of shared and unshared electrons, given above, and with the bivalence of oxygen, which is so essential for the proper understanding of many organic compounds, they emphasize, again, the importance of considering polar valences as genuine valences.

I have already referred to the conduct of trimethylamine oxide as indicating a double union between the nitrogen and oxygen. This is evidently a semipolar union:



A similar union was assumed by Kendall and myself for an alicyclic diazo compound,  , in 1926, but the designation "semipolar" was not used.

Harrison, Kenyon and Phillips, in 1926, obtained a sulfinate with an optical rotation of 130°. Since the compound contains no atom except sulfur which could be considered asymmetric, they assign to their compound the formula,  , with a semipolar union between the sulfur and the oxygen.

Abegg pointed out, more than twenty years ago, that the maximum valence of an element toward oxygen is equal to or greater than that toward hydrogen and that the sum of the

two valences is eight: *e.g.*, CH_4, CO_2; NH_3, N_2O_5; H_2S, SO_3; HCl, HO–ClO_3. Abegg called the valences toward hydrogen "principal" and those toward oxygen, "contra" valences.

The principles which have been given furnish a very simple explanation of Abegg's rule. Since the hydrogen nucleus has only one positive charge, it readily separates from a covalence with an atom having a kernel with a multiple charge, leaving the pair of electrons with the latter. It can not form semipolar unions. Consequently, an atom with a kernel of multiple charge can add hydrogen atoms, to form electrically neutral molecules, only in sufficient numbers to balance the kernel charges in accordance with the principle of the sum of shared and unshared electrons. This gives the hydrogen compounds listed above. When such atoms add another hydrogen atom, as in the ammonium ion, NH_4^+ the ion is stable only in the absence of other ions which unite with hydrogen to form compounds that ionize slightly. The ammonium ion, for instance, is very unstable in the presence of hydroxyl ions, or even in the presence of acetate ions or the ions of weak acids.

When an atom of multiple charge unites with oxygen, both atoms cling firmly to the electrons of the covalence and the oxygen atoms may, also, form semipolar bonds. For one or both reasons, the valence toward oxygen may equal the number of valence electrons. That it does so is seen in the oxygen compounds listed.

This illustrates, again, the fact that for the true valence of an atom we must consider both the covalences and the polar valences. By doing this, the older ideas of valence are easily and simply reconciled with the views of Lewis and Langmuir. But we should remember that an atom with a semipolar valence is not an atom with a free valence which can be exercised without satisfying, in some way, the corresponding polar valence of the atom with which it is associated.

In 1922, Stern and Gerlach demonstrated that isolated silver atoms are magnetic. Using similar methods, Kunz, Taylor and Phipps demonstrated, in 1926, that atoms of

sodium and potassium are magnetic. Still more recently, Phipps and Taylor have shown the magnetic character of hydrogen atoms and that the properties are, at least approximately, those of a Bohr magneton. This gives very welcome evidence in support of Bohr's theory at a time when some physicists are inclined to abandon it.

Hydrogen molecules are not magnetic and Lewis has often pointed out that paired electrons are not usually magnetic. These facts have suggested a picture of the hydrogen molecule recently presented at a meeting of the National Academy of Sciences. Two hydrogen atoms in the neighborhood of each other would orient themselves as magnets with their electrons rotating in the same direction, in parallel planes. The repulsion between the electrons and also that between the nuclei, would prevent the atoms from coalescing to a molecule. Under the influence of a metal, as in Langmuir's blowpipe, we may suppose that the two nuclei fall into a position between the planes of the two rotating electrons, which take opposite directions, and that the orbits are decreased in size. The fall of the electrons toward the nuclei might account for the dissipation of energy which occurs when two atoms of hydrogen unite to form a molecule.

If we could find some catalyst which would cause the four nuclei (protons) and two of the electrons of two hydrogen molecules to coalesce to a nucleus with two positive charges, leaving the other two electrons to rotate about this, we should have a helium atom. Since the mass of a helium atom is eight tenths of a per cent. less than that of two hydrogen atoms the dissipation of energy in such a condensation would be tremendous and of a wholly different order from that involved in the formation of a hydrogen molecule.

I am well aware that this address has been full of pictures and that the newer pictures can have no permanent value unless a way can be found to test them experimentally and mathematically. My excuse for presenting them is that the greatest advances in chemistry for the last century and a

quarter have been directly dependent on the picture of atoms given us by Dalton and the pictures of structure given us by Frankland, Couper, and Kekulé. I think no chemist would claim that our science could have secured its present insight into the nature of chemical phenomena without the aid of those pictures.

UNIVERSITY OF ILLINOIS,
URBANA, ILL.

CHRONOLOGY

1729—Gray—Conductors of electricity.
1735—Dufay—Vitreous and resinous electricity.
1747—Franklin—Positive and negative electricity; letter to Peter Collison in England.
1786—Galvani—Current electricity; soon after Volta's batteries.
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1807—Davy—Older electrochemical theory. *Phil. Trans.*, 1807, 1.
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1914—L. W. Jones—Amine oxides. *J. Amer. Chem. Soc.*, Vol. XXXVI., p. 1268 (1914).
1915—Noyes, Potter—Configuration due to polar valence. *J. Am. Chem. Soc.*, Vol. XXXVII., p. 189 (1915).
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DEVELOPMENT OF BOTANY IN THE UNITED STATES

By JOHN M. COULTER

(*Read April 29, 1927*)

The period covered by the present celebration includes the whole history of the origin and development of botany in the United States. In outlining this history I shall not include citations from bibliography, for the names of investigators who have made important contributions to our botanical progress are numerous, and a selection would be difficult. I shall attempt, therefore, only to present in outline the development of the subject, from its start in this country to its present status.

At the beginning of the period we are considering, botany was represented only by the random collection of plants from a virgin flora. At first these collections were not studied or published in this country, for there were no botanists trained sufficiently for this work. The plants were sent or taken chiefly to England and France, and described and published in those countries. Presently American botanists began to develop, and investigation started in this country.

This first phase of botany is known as taxonomy, and it was the only phase for many years in this country. This was natural, for our flora was being explored for the first time, and the enormous extent of the territory to be covered led to the organization of many exploring expeditions, each securing a wealth of new material. At that time, therefore, botany in the United States consisted only of the collection and naming of plants. Even this phase of our subject was much restricted. Only the higher plants were collected and named, and since the flower is the most used structure in the classification of the

higher plants, botany at that time came to mean to the general public the study of flowers. In this country, therefore, taxonomy at that time was not only the only phase of botany studied, but even that phase was in its infancy. It was in this period that our manuals of botany began to be published, registering the current knowledge of the conspicuous flora of various regions. This phase of our subject may be defined as the assembling of material for subsequent investigation.

It must not be supposed that taxonomy stopped its development when other phases of botany started. It calls for more critical work now than ever before, for the accumulating body of morphological data is constantly shifting classification. Taxonomy must include the last expression of our knowledge of plants, so that taxonomists are engaged in keeping their phase of botany up to date.

The next phase of botany to begin its development was morphology, the study of structures. This start was made under the influence of European contacts. In those days botany was progressing rapidly in Europe. The American botanists visited European laboratories, and returned to this country equipped for a new kind of investigation. Morphology should probably be regarded as the most fundamental subdivision of botany, for it treats of the structures of plants and their development, and upon these data taxonomy must build. At first morphology dealt only with the forms of mature plants and their organs. Everything about a plant was referred to a few categories, the favorite ones being root, stem, and leaf. The work of this primitive morphology was to strip off all disguises and to reveal the real nature of the structure. This early phase of morphology was dominated by the doctrine of metamorphosis, meaning that root, stem, and leaf may become so modified that their real nature is not obvious.

Gradually the modern phase of morphology began to develop. Instead of studying mature structures alone, their development began to be investigated, following much earlier investigations in certain European laboratories. In this work,

the life histories of plants began to be traced, from the egg to maturity. In connection with this work, the lower plants began to attract attention, until finally all groups, from algæ to seed plants, were being investigated. This has furnished the basis of many conclusions concerning the relationships of the great groups, and the evolution of the plant kingdom as a whole.

This development of morphology began the segregation of botanists that increased rapidly. Botanists were grouped as taxonomists and morphologists, and neither group shared in the work of the other. In fact, at that time the college courses in botany were chiefly morphology, and attention to taxonomy began to decline.

The next segregate in botany was physiology. This phase of work was also introduced into this country through contact with European laboratories. Nothing was known of the activities of plants except what had been learned through agricultural and horticultural operations. The physiology of plants began to be studied long after human and animal physiology was under way. The functions of the different plant organs were gradually discovered, and a new technique of plant investigation became established. Plants were not merely observed, as in the case of taxonomy and morphology, but were put under experimental control, and their various activities evoked. The important general functions which engaged physiological research were the absorption of material and of energy from the outside world; the transfer of water through the plant body; transpiration, by means of which water is lost from the plant surface; photosynthesis, by which carbohydrates are manufactured; digestion and assimilation; respiration, by means of which energy is liberated for the activities of the body; growth; and movement. Gradually our knowledge of these fundamental processes increased, until now an immense body of facts has been accumulated, awaiting explanation through more highly developed methods of experimentation. It is an interesting fact that physiology became so much segregated from the other fields of botany that

most of the investigations were carried on with no knowledge of the structure of the machinery doing the work.

Next in historical succession, ecology emerged, with its interest in the effect of environment on plants. This is really a phase of plant physiology. This phase of botany is about 30 years old, but it has had a very rapid and extensive development. Three rather distinct lines of investigation have been developed. One deals with the reactions of plants, tissues, and organs to their environment, a phase of the subject called physiologic ecology. A second line of ecological investigation has to do with the origin, development, and life relations of the plant communities, often called plant societies, and is called physiographic ecology. The third line of investigation is concerned with the great forest, grass, desert, and other formations of the earth in relation to climate, and is called geographic ecology. This field also became so completely segregated from the other phases of botany that ecologists observed the responses of plants without any knowledge of the physiology involved, or of the structures responding.

The next segregate of the botanical field was plant pathology, which deals with plant diseases, chiefly those inflicted by other plants. In the meantime bacteriology had developed because of its relation to human diseases, and was hardly considered as a part of botany. When plant pathology began to develop, however, as a distinct field of investigation, bacteria were inevitably included, at least so far as bacteria are related to plant diseases. It is of importance to note that plant pathology, as first developed, was interested solely in the parasites inducing disease, discovering them and investigating their life histories, but really paid little attention to the diseases induced. In other words, it paid little attention to the patient whose diseased condition belonged to physiology.

Finally, the last segregate, which really began with the present century, was plant breeding, which in its scientific aspects is known as genetics. As in the case of the other segregates, breeding was carried on with most complex material, with little knowledge or appreciation of the structures involved.

All of these phases of botany developed independently. As a result, not only were the phases of botany segregated, but the botanists also. Taxonomists, morphologists, physiologists and ecologists represented distinct and too often rival groups. The prominent advantage of this segregation of subjects was the development of the technique of each field. One had to be a comparatively narrow specialist to develop technique to its limit. The disadvantage of the segregation of botanists was the lack of coöperation. Taxonomists did not know morphology or physiology. Morphologists cared nothing for taxonomy, and still less for physiology. Physiologists cared nothing about either; and so on down the list of segregates. This segregation was for a time very complete, so that the interests of one group would not have been affected if none of the other groups had existed. This may be called the monastic phase of botany.

The transformation of botany from a list of segregates to a synthesis of all the sciences was hastened by our experience during the Great War. In that emergency every phase of botany was called upon to contribute information. It was a revelation to discover that every phase proved to be useful. This coöperation of botanists in the general cause of public service was carried over to coöperation in research. The different groups of botanists came into contact, and even interlocked in their work, so that botany began to develop as an increasing synthesis, rather than an increasing disintegration. This is the present status of botany, and the culmination of its history, which started with one phase, then multiplied independent phases, then coördinated them and all other sciences in the attack on problems, may be described briefly. There are certain outstanding features which will serve as illustrations.

As a natural result of our war experiences and of the importance of our agriculture, one of the features of botanical research to-day is to attack problems that are fundamental in connection with some important practice. In the older days, botany was regarded as the most impractical of all the sciences.

It was really regarded as a pastime rather than a profession. The outstanding illustration, of course, is the increasing attention given to the problems that underlie agriculture; but there are many other practices also which are bedded in botanical investigation. It was this tendency that was stimulated by the war. It is so strong at present that I do not believe it will ever subside, but it should be understood. The great objective of research, of course, is to extend the boundaries of knowledge. The tendency to which I refer merely means that experience developed in connection with an important practice has suggested fundamental problems. In fact, among our most fundamental problems are those that have been suggested by experience. It must be confessed that certain botanists still resent the inclusion of what they call practical problems among our research problems. It is very common to distinguish the two groups of problems as pure and applied science. The fact is that pure science is often immensely practical, and that applied science is often very pure science. Between the two there is no dividing line. Practical problems are not a detriment to botanical science. In fact they incidentally strengthen its claim on public interest as a science that must be promoted. It will develop a far greater opportunity for research than has been possible heretofore. I regard this so-called practical tendency in research as being entirely in the interest of research in general, in increasing the range of fundamental problems, in contributing a powerful stimulus, and in securing general recognition of the importance of research. I look upon this present tendency, therefore, as one to be encouraged rather than slighted.

A second feature that characterizes botany to-day, and which I regard as more important than the preceding feature, is the increasing realization of the fact that botanical problems are synthetic. Until recently, a problem would be attacked from a single point of view, with a single technique, and conclusions reached that seemed rigid. This was the natural situation when botany was pigeonholed in rigid compartments.

In plant morphology, for example, structures were described, with no adequate conception of their functions. Plant physiologists, on the other hand, would describe functions, with no adequate knowledge of the structures involved. Ecologists often described responses, with no adequate knowledge of either structure or function. The same condition obtained in the other segregates of botany. The fact is that this kind of segregated research resulted in the cataloguing of facts, to be coördinated later. This coördination has now begun, and is one of the outstanding features of botanical investigation to-day. Around each bit of investigation, with its single point of view and single method of attack, there is developing a perspective of other points of view and other methods of attack. We realize now that plants are synthetic, and that is quite a notable advance from the distant time when we thought of them only as objects subservient to laws of nomenclature. This increasing synthetic view is resulting in a proper estimate of problems. In fact, the synthetic view has developed about our problems the atmosphere in which they actually exist.

A third feature that is becoming increasingly prominent is a recognition of the fact that structures are not static; that is, inevitable to their last detail. This is so fundamental in connection with the future development of botany that I wish to emphasize it by a few illustrations.

The old method of morphology in recording the facts observed in connection with the development of the embryo of seed plants, was to record not only every cell division, but also the plane of every cell division. The conception back of such records was that the program of ontogeny was fixed to the minutest detail. It is probably true that such a structure is about as uniform in its development as any structure can be; but it has now become evident that many of the details recorded were not significant. Instead of recording them as of equal value, we are now trying to distinguish those that are relatively fixed from those that are variables.

In the same way, much of the older work in plant anatomy

must be regarded as records of details whose relative values were unknown. Even the structures involved in vascular anatomy are found not to be static, but many an evolutionary connection has been formulated on the conception of the absolute rigidity of such structures in their minutest detail. This conception has made it possible, of course, to develop as many static opinions as there are variables in structure.

Perhaps the greatest mass of details has been accumulated by the cytologists, in connection with their examination of the machinery of nuclear division and nuclear fusion. In no other field has the conception of rigidity of the structures involved become more fixed, even to the minutest variation in form and position. The time has come when even the recorded facts of cytology are being estimated on the basis of relative values; that is, the inevitable things are being differentiated from the variables.

The same situation is developing in the field of genetics. We all recall the original rigidity of the so-called laws of inheritance. It was natural to begin the cultivation of this field with the conception that the programme of heredity is immutable, and that definite structures are inevitable, no matter what the conditions may be. There was probably more justification for this conception in this field, on the basis of the early investigations, than in any other, but experience has begun to enlarge the perspective wonderfully. The rapidly accumulating facts are becoming so various that consistent explanations require a high degree of mental agility. More fundamental, however, is the recognition of the fact that the problem of heredity involves not only germinal constitution, which gives such rigidity as there is, but also the numerous factors of environment. In other words, such problems have become synthetic in the highest degree, making possible results that are anything but static.

In considering these illustrations of the tendency to recognize that facts are not all pigeonholed and of equal value, it is becoming more and more obvious that our botanical problems are in general the application of physics and chem-

istry to plants; that laws, when we really discover them, are by definition static, but that their operation results in anything but static structures. In other words, structure must respond to law, but the particular law that is gripping the situation may be one of many.

We have arrived at the stage, therefore, when we are beginning to pool our knowledge and our technique. We are organizing separate camps into a single army. We must know the relations of the plants we work with; we must know their structure; we must know how they live and work; we must know how they are affected by their environment, and so on.

One of the striking features in the recent development of botany in this country is the growing recognition of the fact that the science of botany is fundamental in all practice that has to do with the handling of plants. This has brought it into connection with the human interest. This does not mean a revolution in plant research, but it does mean that our results are not allowed to remain in cold isolation, entirely unrelated to the activities of life. There is a growing recognition of the fact, therefore, that the scientific handling of plants is a great public service. This does not mean less science, but more science, science with the additional stimulus of a new motive.

Until recently the scientific and practical phases of botany were completely divorced. As a consequence, the science was not practical, and the practice was not scientific. At last it came to be recognized that practice by itself is sterile, and that it needs a continuous discovery of new knowledge to apply. This new botanical perspective, therefore, might be described as practice based on science, and science that illuminates and extends practice. As already stated, the great stimulus that really gave birth to this modern aspect of botany was given by the Great War. At that time a large number of emergency problems came to the front. Many of these emergency problems had to do with raw products used in necessary manufactures, essential drugs, and timbers for various purposes, notably for aëroplanes. These were certainly practical matters of the first importance, but the practi-

cal man could not solve these problems. It was the botanist with scientific training to whom the appeal was made, and his training was usually sufficient to supply the necessary information as the basis of new practice. His contribution was a greater one than meeting the immediate need. We had been dependent upon other countries for most of these raw products, and now the supply had been cut off. The botanist showed that our own flora contains them all, so that his contribution was the development of our own natural resources, which means a greater national self-dependence.

It should be recognized also that practice makes its contribution to progress, for under the pressure of necessity it is practice that suggests the problems. In this way the cooperation of botanical science and practice became established. One of the outstanding features of our present status, therefore, is the increasing interest in applying the results of our research to the improvement of practice. This has become so well recognized that all the great industries that are concerned with plants or their products have provided for botanical research in connection with their problems.

In conclusion, I may give what may be called the modern creed of botany. I shall put it under three articles, in the order of what I conceive to be their relative importance. The service of botany to-day is (1) to understand nature, that the boundaries of human knowledge may be extended; this may be called pure science; (2) to apply this knowledge to the service of man, that his life may be fuller of opportunity; this may be called applied science; and (3) to use the method of science in training man, that he may solve his problems and not be their victim.

LAMARCK'S EVENING PRIMROSE (*OENOTHERA LAMARCKIANA* SERINGE) WAS A FORM OF *OENOTHERA GRANDIFLORA* SOLANDER[1]

By BRADLEY MOORE DAVIS

(*Read April 29, 1927*)

Fifteen years ago the writer (Davis, 1912) published a paper which presented evidence that the *Oenothera* grown in the gardens of the Muséum d'Histoire Naturelle at Paris about 1796, described by Lamarck (?1798) under the name *Aenothera grandiflora* and renamed by Seringe (1828) *Oenothera Lamarckiana*, was a form of *Oenothera grandiflora* Solander (1789), introduced into England in 1778 from Alabama. If this identification is correct it follows that the name *Oenothera Lamarckiana* Seringe becomes a synonym of *Oenothera grandiflora* Solander (*O. grandiflora* "Aiton"). It also follows that the plant known in genetical literature from the studies of Professor de Vries as *Oenothera Lamarckiana* is not Lamarck's plant of 1796. The material of de Vries's cultures is very different from *Oenothera grandiflora* Solander. If allowed to keep the name *Lamarckiana* it must be understood as *Oenothera Lamarckiana* of de Vries; it is not *Oenothera Lamarckiana* Seringe.

The evidence offered in my paper of 1912 was chiefly from data obtained through the courtesy of M. François Gagnepain who most kindly answered many specific enquiries on the history of specimens preserved in the herbaria of the Muséum d'Histoire Naturelle and concerning characters not shown by photographs which he obtained for me. Miss Alice Eastwood also reported to me observations upon these specimens.

[1] Genetical Studies on Oenothera—XV. Papers from the Department of Botany, University of Michigan, No. 266.

Professor de Vries (1914*a*) quickly replied to my paper after a visit to Paris in 1913. He discovered some changes in the mounting of important specimens in the Herbarium of Lamarck since an earlier examination in 1895 on which was based the account in Die Mutationstheorie 1901, pp. 316, 317. As a result of this second examination there can be no misunderstanding of de Vries's opinion as to what is the type of *Oenothera Lamarckiana* Seringe, and we have also very positive conclusions on the identity of other interesting material in the collections at Paris. Thanks to his published photographs of these sheets further confusion will be impossible and botanists may now make for themselves the observations that will in the end determine their judgment of the soundness of de Vries's interpretations and of the value of the exceptions that may be taken to them.

Professor de Vries did not modify his views in any important respect and strongly opposed my conclusions, being very critical of the fact that I relied upon the observations of others in building my argument. At the time I could only answer with a brief review (Davis, 1915) in which, however, I expressed disagreement and pointed out that de Vries gave us no descriptions of pubescence on sepals, stems or capsules although *grandiflora* and *Lamarckiana* may be readily distinguished by peculiarities of pubescence and such characters are generally clearly shown on herbarium material.

It was not until the summer of 1925 that I had an opportunity to visit Paris for a personal study of the material under discussion, and later to examine herbaria in England for data on the time of appearance and distribution in England of *Oenothera biennis* Linnaeus, *O. grandiflora* Solander, and *O. Lamarckiana* of de Vries. The results of the studies in England have been published (Davis, 1926) and certain of their conclusions will be further discussed in relation to the subject of this paper. It is a pleasure to express my deep obligations to M. Gagnepain for the courtesies shown me during the several days spent at the herbarium of the Muséum d'Histoire Naturelle. His patience in answering my many questions

concerning dates, handwriting, and the history of sheets, information which a well-organized herbarium has to give, made progress possible in matters about which a visitor generally knows but little.

The material of this paper will be presented under the following headings:

1. Comparison of the characters, likely to be shown in herbarium material, which distinguish *Oenothera grandiflora* Solander from the *Lamarckiana* of de Vries.
2. Discussion of Lamarck's description in the Encyclopédie Méthodique Botanique (?1798) of *Aenothera grandiflora*, renamed by Seringe (1828) *Oenothera Lamarckiana*.
3. Description and discussion of the specimen held by Gagnepain and others at the Muséum d'Histoire Naturelle to be the type of Lamarck's species. Plate I.
4. Description and discussion of the specimen which de Vries believes to be the type of Lamarck's species. Plate II.
5. Discussion of a specimen from the herbarium of Abbé Pourret considered by de Vries to be *Lamarckiana*. Plate III.
6. Description of a particularly fine specimen of *Oenothera grandiflora* grown at Paris in 1815, from the herbarium of Jacques Gay. Plate IV.
7. Discussion of a specimen from the herbarium of André Michaux held by de Vries to be *Lamarckiana*. Plate V.
8. A specimen of Michaux's, possibly *Oenothera suaveolens* Desfontaines.
9. The probable origin of *Oenothera Lamarckiana* of de Vries.
10. Summary.
11. Literature cited.

1. Comparison of the Characters, Likely to be Shown in Herbarium Material, which Distinguish *Oenothera grandiflora* Solander from the *Lamarckiana* of de Vries

Oenothera grandiflora Solander was discovered by William Bartram in 1776 near Tensaw, Alabama, on an expedition undertaken at the request of John Fothergill, M.D., MacDougal (1905, p. 7). Solander's original description in Aiton's "Hortus Kewensis," 1789, from material grown at Kew states that *O. grandiflora* was introduced by John Fother-

gill in 1778. The species is well known to such geneticists as have in recent years grown material from Dixie Landing (near Tensaw), Alabama, a locality discovered in 1904 by Tracy who made a successful search for the plant in the interests of MacDougal. Tracy also made later visits to Dixie Landing sending seed to those who wished it. Professor de Vries (1914*a*, p. 348) visited Dixie Landing in 1912, unfortunately finding the station "in a most desolate condition" and without pure *grandiflora*, but the earlier collections of Tracy have given American geneticists lines of *grandiflora*, breeding true and typical of the species, and the characters of the plant are familiar to them.

There are given below the characters, likely to be shown in herbarium material, which distinguish *Oenothera grandiflora* Solander from the *Lamarckiana* of de Vries:

Oenothera grandiflora	*Oenothera Lamarckiana*
Flowering shoots. Generally with numerous approximate branches.	Flowering shoots. Sparsely branched above or not at all.
Stems. Sparsely pubescent, green above, reddish below, papillae never red over green portions of the stem.	Stems. With heavy pilose and puberulent pubescence, long hairs from numerous red papillae over green portions of the stem.
Leaves of upper foliage. Smaller, lanceolate, in some forms broad at base, distinctly petioled, plane, with less pubescence.	Leaves of upper foliage. Larger, broader, short-petioled or almost sessile, the larger crinkled, with more evident pubescence.
Inflorescence. More open, narrower bracts.	Inflorescence. Dense spike, flat topped, broader bracts.
Buds. Buds about 9 cm. long. Long slender hypanthium and slender cone.	Buds. Buds about 8 cm. long. Stronger hypanthium and much wider cone.
Sepals. Glabrous or sparsely pilose.	Sepals. With heavy pilose and puberulent pubescence.
Sepal tips. 8–10 mm. long, attenuate and with relatively little pubescence.	Sepal tips. 6–8 mm. long, thicker and with a very heavy pilose pubescence.
Hypanthium. Of open flowers 4–5 cm. long, with light pilose and puberulent pubescence.	Hypanthium. Of open flowers 4 cm. or less, with heavy pilose and puberulent pubescence.

Ovary. Glabrous or almost glabrous.	Ovary. Strongly pilose and puberulent.
Capsules. 2.5–3 cm. long, glabrous.	Capsules. 2–2.5 cm. long, strongly pilose and puberulent.

It will be noted that the most striking differences between *grandiflora* and *Lamarckiana*, likely to be shown by herbarium material, are those of pubescence on stems, sepals, ovaries and capsules. In my experience of many years with *grandiflora* and *Lamarckiana* I have found these characters of pubescence wholly stable and I am confident that they may be relied upon to separate these species even when little other evidence is available.

Next in importance to pubescence on herbarium material of these two species are such differences as the branching of the flowering shoots, the attachment and form of the leaves, the form of the buds, the form and length of the sepal tips, the length of the capsules. Sometimes certain of these characters are clearly shown, sometimes they are obscured by the drying of the specimen, but pubescence can almost always be determined however poorly the specimen may be preserved. For example the type specimen of *Oenothera grandiflora* in the Herbarium of the British Museum is so much mutilated that few of its characters are clearly shown, but of these the pubescence is unmistakably that of *grandiflora*. It is to me most surprising that Professor de Vries should so completely ignore the evidence from pubescence which stands out so clearly.

2. Discussion of Lamarck's Description in the Encyclopédie Méthodique Botanique (?1798) of *Aenothera grandiflora*, Renamed by Seringe (1828) *Oenothera Lamarckiana*

The following is the description of Lamarck's plant written by Lamarck (?1798) for the Encyclopédie Méthodique Botanique. The letters V.S. (vidi siccum) at the end of the diagnosis mean that the description was based on dried

material. I know of no evidence that Poiret wrote the description as believed by de Vries (1914*a*, p. 352 footnote), but he is known to have written later volumes of the encyclopedia.

"12. Onagraire à grandes fleurs. *Aenothera grandiflora* (n). *Aenothera foliis integerrimis, ovato-lanceolatis; petalis integris, capsulis glabris.*

"Cette espèce paroît se rapprocher, par son port, de *l'aenothera longiflora;* mais elle en diffère par plusieurs caractères frappans, sur-tout par ses tiges rameuses, ses pétales entiers, ses fruits lisses & courts.

"Ses tiges s'élèvent à trois ou quatre pieds de hauteur. Elles sont cylindriques, munies de quelques poils rares, d'un rouge brun, divisées en rameaux nombreux, étalés. Les feuilles sont vertes, alternes, ovales, lancéolées, lisses & glabre des deux côtés, très-entières; les feuilles du bas sont pétiolées & munies de quelques dents à peine sensibles. Celles qui accompagnent les fleurs sont plus étroites, plus aiguës & sessiles.

"Les fleurs sont terminales, & forment, par leur disposition, une panicule étalée; elles sont axillaires, solitaires, mais très-rapprochées. Le calice est jaune, muni d'un tube un peu plus long que la corolle, qui se divise en quatre folioles lancéolées, élargies à leur bâse, aiguës à leur sommet, terminées par un filet court, sétacé. La corolle est jaune, composée de quatre pétales ovales, très-grands, entiers, arrondis, presque aussi long que le tube calicinal, retrécis à leur bâse en forme de coin. Les anthères sont longues, linéaires. Le fruit est une capsule courte, cylindrique, glabre, tronquée, légèrement quadrangulaire, n'ayant qu'environ le tiers de longueur du tube calicinal. Cette espèce est originaire de l'Amérique septentrionale. On la cultive au jardin du Muséum d'Histoire naturelle. (V. S.)"

I give also the short diagnosis by Seringe (1828) of *Oenothera Lamarckiana* for comparison with the longer description of Lamarck:

"OE. Lamarckiana (Ser. mss.) caule ramoso, *foliis integerrimis ovato-lanceolatis*, *petalis integris* magnis, *capsulis glabris* cylindrico-tetragonis brevibus. 2 in Americâ sept. OE. grandiflora Lam. dict. 4. p. 554. *non Ait. Fl. flavi."

I have italicized phrases that are the same as those in the latin introduction in the description of Lamarck, showing that

Seringe had little or nothing to add to the original description. Seringe gives *O. grandiflora* Lamarck as a synonym together with the comment that the species is not the *grandiflora* of Aiton. This was of course his reason for renaming the plant.

Professor de Vries (1914*a*, p. 352) is insistent "that in systematic researches of this kind more value is to be attached to published diagnoses and descriptions than to material preserved in a herbarium." We will then take the description of Lamarck's and in a translation italicize points that do not agree with the *Lamarckiana* of de Vries but are characteristic of *Oenothera grandiflora* Solander:

"Leaves entire, ovate-lanceolate, petals entire, *capsules smooth*.

"This species resembles in habit *Oenothera longiflora*, but differs in several striking characters, in particular by the branching stems, entire petals, *smooth* and short fruits.

"The stems rise three or four feet. They are cylindrical, *almost glabrous, reddish brown with numerous spreading branches*. The leaves are green, alternate, oval-lanceolate, *smooth* and glabrous on both sides, entire; lower leaves are petioled and slightly toothed. The bracts are more narrow, more pointed and sessile.

"The flowers form a terminal broad cluster; they are single in the axil but very much crowded. The calyx is yellow, with a tube a little longer than the four sepals, broad at the base, pointed at the tip and ending in a short thread, *bristle-like*. The corolla is yellow, of four petals, oval, very large, entire, rounded, almost as long as the calyx tube, tapering to a narrow base. The anthers are long, linear. The fruit is a short capsule, cylindrical, *smooth*, truncate, slightly four-angled, about one-third of the length of the calyx tube."

Oenothera Lamarckiana of de Vries has stems and capsules with a very heavy pilose and puberulent pubescence; *grandiflora* has a stem with very few hairs and capsules characteristically glabrous. The stems of *Lamarckiana* are covered with numerous red papillae, conspicuous against the green surface; the stems of *grandiflora* have few papillae never red over green portions of the stems which later become reddish brown, and there are numerous approximate branches. The leaves of *Lamarckiana* are crinkled and the upper leaves are sessile or almost sessile; those of *grandiflora* are plane and

petioled. The sepal tips of *grandiflora* are thin and bristle-like, being longer and more delicate than those of *Lamarckiana*. When Lamarck states so clearly that the stems of his plant are almost glabrous and repeatedly that the capsules are glabrous we can hardly suppose that he made a mistake, but these characters alone would separate his plant from the material of de Vries.

There is only one point in Lamarck's description that seemingly does not hold for *Oenothera grandiflora* Solander. Lamarck describes the fruits of his plant as short and about one-third the length of the calyx tube. The hypanthium of *grandiflora* is often 5 cm. long and that of *Lamarckiana* about 4 cm.; in both species full sized capsules are more than one-third the length of the calyx tube. The capsules of *Oenothera grandiflora*, 2.5–3 cm. long, are not short; they are distinctly longer than those of de Vries's plant, 2–2.5 cm. This point in Lamarck's description troubled me from the first and I was forced to suggest that Lamarck might have described immature or partially pollinated capsules on dried material, or that he had some short capsuled race of *grandiflora* (Davis, 1915, p. 61).

The matter became clear, however, when in the collections of the Muséum d'Histoire Naturelle at Paris I saw material of *Oenothera longiflora* with which species Lamarck directly compares his plant in the second paragraph of the description of *Aenothera grandiflora*,—"This species resembles in habit *Oenothera longiflora*, but differs in several striking characters, in particular by the branching stems, entire petals, *smooth* and *short* fruits." Lamarck in his description of *Aenothera longiflora* Lin., which was No. 4 of the species described in the Encyclopédie Méthodique Botanique, does not give measurements of the capsules, but a specimen from the herbarium of Abbé Pourret in the collections of the Muséum d'Histoire Naturelle shows a capsule 4 cm. long and probably not yet full grown since it is only six capsules removed from the flowers. Another specimen from Brazil (C. Caudichaud, 1883) has capsules that must have been 5 cm. long. The buds,

ovaries, capsules and stems of these specimens are covered with a dense pilose and puberulent pubescence in most striking contrast to the smoothness which Lamarck noted for his *Aenothera grandiflora.*

Therefore, when Lamarck wrote of the "short fruits" of *Aenothera grandiflora* he meant short in comparison with the fruits of *Aenothera longiflora*, and in this sense the capsules of *Oenothera grandiflora* Solander are short. With this point explained there is nothing in Lamarck's description of his plant which is not true for *Oenothera grandiflora* Solander.

3. Description and Discussion of the Specimen Held by Gagnepain and Others at the Muséum d'Histoire Naturelle to be the Type of Lamarck's Species. Plate I

It is regrettable that there should be disagreement as to which of two specimens in the Herbarium of Lamarck is the type of Lamarck's species, but it is most fortunate that these two specimens can scarcely be distinghished and that there can be no mistaking their affinities with *Oenothera grandiflora* Solander. The nature of their pubescence together with other characters makes it certain that they cannot be identified with the *Lamarckiana* of de Vries's cultures. Also, they agree perfectly with Lamarck's description which, as we have seen, the *Lamarckiana* of de Vries does not. Therefore discussions as to whether Lamarck based his description on one or the other or perhaps upon both specimens have no bearing on the contentions of this paper.

I follow the opinion of M. Gagnepain and others of the staff in the herbarium of the Muséum d'Histoire Naturelle and attach greater importance to the specimen shown in Plate I which will be referred to as the first specimen. De Vries (1914*a*) and Blaringhem (1914) hold that the specimen shown in Plate II is the type. Both sheets bear the handwriting of Lamarck and the specimens must have been known to him and both may have been concerned with the description of *Aenothera grandiflora* Lam. with which each agrees.

The sheet shown in Plate I has in the handwriting of Lamarck "Aenothera . . . [grandiflora] . . . nova spec. flores magni lutei, odore grato, caulis 3 pedalis." As de Vries (1914*a*, p. 353) remarks, the words "*odore grato*" point to *O. grandiflora* Ait., which has fragrant flowers, while the flowers of *O. Lamarckiana* Ser. are almost without odor. De Vries suggests that the specimen was perhaps a chance hybrid from the Jardin des Plantes about which Poiret (1816) expressed the opinion, "L'Oenothera grandiflora, no. 11 est la même plante que celle d'Aiton, Hort. Kew., 2, pag. 2." De Vries writes of the specimen, "Personally, it impressed me as having been brought into the herbarium of Lamarck only later on, and as having been placed in the cover of *O. grandiflora* Lam. with a doubt shown by the placing of the name in brackets." Others do not share this opinion. It is curious that Professor de Vries is satisfied that this specimen is not the *Lamarckiana* of his cultures and yet has such faith in the second specimen since I expect to show that the second specimen, figured on Plate II, does not differ from the first in any significant respect.

I shall give again my reasons for identifying the specimen figured in Plate I with *Oenothera grandiflora* Solander, since it is important that they be understood in connection with the description of the specimen shown in Plate II.

1. *Stem.*—The specimen, Plate I, exhibits the rather dense branching, which I have termed approximate, that is characteristic of certain forms of *grandiflora* and is not shown in the long sparsely branched stems of de Vries's *Lamarckiana*. The stem is lightly puberulent with occasional long hairs from small papillae; that of *Lamarckiana* would be covered with a heavy pilose and puberulent pubescence, the long hairs from large red papillae. The color of the stem has become that uniform brown found in old herbarium material.

2. *Foliage.*—The leaves, broadly elliptical or lanceolate, have short but distinct petioles; they are not sessile or almost sessile as those of *Lamarckiana*. This foliage may be readily matched by numbers of specimens of *grandiflora* collected in

PLATE I.—Lamarck's plant, held at the herbarium of the Muséum d'Histoire Naturelle to be the type of *Oenothera Lamarckiana* Seringe. Lamarck is believed to have used this material in writing his description with which the material fully agrees. This is a specimen of *Oenothera grandiflora* Solander.

Alabama; it represents neither the broader or nor the narrower-leaved forms in the range of variation exhibited by this species but is nearest to the intermediate condition.

3. *Inflorescence.*—The inflorescence is more open that that of *Lamarckiana* and with narrower, short-petioled bracts, in these respects agreeing with *grandiflora.*

4. *Buds.*—The buds have the slender cones and attenuate sepal tips distinctive of *grandiflora*; those of *Lamarckiana* have stout cones and shorter sepal tips. The pubescence of the sepal tips is like that of *grandiflora*, puberulent and lightly pilose, in amount scarcely one-third that of *Lamarckiana.* The pubescence of the middle region of the bud cone, as in *grandiflora*, is sparsely puberulent and without long hairs; this region in *Lamarckiana* would have a heavy pilose and puberulent pubescence.

5. *Flowers.*—The flowers have the very long and delicate hypanthia, 4.8 cm. with light pubescence characteristic of *grandiflora*, in contrast to the shorter hypanthium and heavy pilose and puberulent pubescence of *Lamarckiana.*

6. *Ovary.*—Almost glabrous, a few short hairs from small papillae as in *grandiflora*; nothing of the very heavy pilose and puberulent pubescence characteristic of *Lamarckiana.*

7. *Capsules.*—There are no capsules on the specimen but they must have followed the ovary in being glabrous as in *grandiflora.*

There is no distinctive character of de Vries's *Lamarckiana* shown on this specimen, and it differs from this species in pubescence of stem, sepals, hypanthium, ovary (and capsules); the branching of the stem, the distinctly petioled leaves, more open inflorescence; slender bud cones with attenuate sepal tips; long and delicate hypanthium. The specimen presents no character that is not in agreement with *Oenothera grandiflora* Solander.

4. Description and Discussion of the Specimen which de Vries Believes to be the Type of Lamarck's Species. Plate II

This interesting branch which I shall call the second specimen of Lamarck's plant bears a label in the handwriting of Lamarck "d'Amérique sept. Tige rameuse, haute de 3 à 4 pieds." The brown color of the stem and other parts of the specimen and the ragged edges of the petals resemble so closely similar parts of the plant shown in Plate I that the two specimens might have been collected and pressed at the same time and might have passed through the same vicissitudes. It is believed at the herbarium of the Muséum d'Histoire Naturelle that the plant was grown in Paris. It is not another branch of the same plant as the first specimen for there are some differences of detail, but it might have come from the same or a related sowings of seed. This second specimen may have been used by Lamarck in the description of his species together with the first specimen and in this sense it would be type as well as the first specimen. The fact that the matter on the label is present in the published description supports this possibility.

I shall make my description of this second specimen a comparison on the one hand with that shown in Plate I, called the first specimen, and on the other hand with *Lamarckiana* of de Vries's cultures. The branch, as de Vries notes, was apparently gathered late in the season as shown by the loose, irregular inflorescence.

1. *Stem.*—The stem shows the same approximate branching as the first specimen. It is somewhat more heavily puberulent but does not have the heavy pubescence of *Lamarckiana.*

2. *Foliage.*—The leaves and bracts are short-petioled as in the first specimen and not sessile or almost sessile as in *Lamarckiana.*

3. *Inflorescence.*—The inflorescence is loose and irregular as happens late in the season.

4. *Buds.*—The bud cones are somewhat broader than in

PLATE II.—A second specimen of Lamarck's plant, held by de Vries to be the type of *Oenothera Lamarckiana* Seringe. It also may have been used by Lamarck in the writing of his description and in that sense it is the type as well as the first specimen shown in Plate I. This second specimen, like the first (Plate I) is *Oenothera grandiflora* Solander.

the first specimen, but bud cones of oenotheras become relatively broader and shorter towards the end of the flowering season. Sepal tips, where well shown, are attenuate as in the first specimen, more strongly puberulent, but again with scarcely one-third of the pubescence of *Lamarckiana*. The pubescence of the middle region of the cone, as in the first specimen, is sparsely puberulent and with no long hairs, in sharp contrast to the heavy pubescence of *Lamarckiana*.

5. *Flowers*.—Professor de Vries (1914, p. 351) bases his determination of this specimen as *Lamarckiana* on the thickness of the buds and the position of the stigma high above the tips of the anthers. No other characters are discussed by him. The relative position of the stigma is quite the same for both *grandiflora* and *Lamarckiana*. The buds have been described in the paragraph above. The long delicate hypanthia, 4.5 cm., with their light puberulent pubescence agree with the first specimen in sharp contrast to the shorter hypanthia of *Lamarckiana* with their heavy pilose and puberulent pubescence.

6. *Ovary*.—The ovary, as in the first specimen, is almost glabrous bearing only a few short hairs from small papillae, this in contrast to the hairy ovaries of *Lamarckiana*.

7. *Capsules*.—There are no capsules on the specimen but of course the almost glabrous ovary would produce glabrous capsules, as would also be expected of the first specimen, and in agreement with *grandiflora*.

Thus we see that the second specimen of Lamarck's differs from the *Lamarckiana* of de Vries in pubescence of stem, sepals, hypanthium, ovary (and capsule); the branching of the stem, the petioled leaves; attenuate sepal tips; long and delicate hypanthia. It agrees with the first specimen in these same characters except for a slightly greater degree of pubescence over stem and sepal tips, which, however, never resembles that of *Lamarckiana*. I can find no character on this specimen distinctive of *Lamarckiana*; the somewhat stouter bud cones emphasized by de Vries are not more marked than might be expected at the end of a flowering season. In no

important respect does the second specimen differ from the first and, since probably grown in Paris, it seems quite possible that it may have been related to the first specimen. In agreement with the first specimen (Plate I) this second specimen of Lamarck's (Plate II) is clearly of the species *Oenothera grandiflora* Solander.

5. Discussion of a Specimen from the Herbarium of Abbé Pourret Considered by de Vries to be *Lamarckiana*. Plate III

This sheet bears the label "Herb. Mus. Paris" with the statement at the bottom "Collection de l'Abbé Pourret, extraite de l'herbier légué par M. le Dr. Barbier en 1847." On the label in the handwriting of Spach is "*Onagra vulgaris grandiflora* Spach." At the left the clerk of Abbé Pourret wrote "*Oenothera grandiflora* L." which means that Pourret held the specimens to be Lamarck's species. There are two stems and two separate flowers on the sheet, all of the same species, but the two shoots may have been from different plants since the degree of pubescence is not quite the same. De Vries (1914*a*) publishes a photograph of this specimen, but gives no account of its critical characters only affirming that it agrees wholly with the *Lamarckiana* of his cultures. Below are the results of my examination of the material, indicating in all respects a relationship to *Oenothera grandiflora* Solander.

1. *Stem.*—The larger specimen is evidently one of the principal shoots gathered early in the season. Buds in the axils of the leaves indicate that branching might take place later. The stems are puberulent with few long hairs from papillae; there is not the heavy pubescence of de Vries's *Lamarckiana*.

2. *Foliage.*—The leaves, as in *grandiflora*, are lanceolate, plane, and distinctly petioled.

3. *Buds.*—The buds, 9 cm., are very long, the cones slender, the sepal tips, 8 mm., much attenuate, in these points agreeing with *grandiflora*. Sepal tips pilose and puberulent,

PLATE III.—Specimens from the collections of Abbé Pourret believed by de Vries to be *Lamarckiana*. On the contrary their characters are those of *Oenothera grandiflora* Solander.

but with scarcely one-third the pubescence of *Lamarckiana*. Middle region of bud cone sparsely puberulent and as in *grandiflora* with no long hairs.

4. *Flowers.*—The flowers all show very long delicate hypanthia, 5 cm., with light puberulent pubescence, features characteristic of *grandiflora*.

5. *Ovary.*—The ovaries have a light pilose and puberulent pubescence; nothing of the hairiness found in *Lamarckiana*.

6. *Capsules.*—The specimens have no capsules. These would naturally follow the ovary in being almost glabrous.

I can find in these specimens no distinctive characters of de Vries's *Lamarckiana*. On the contrary they show the following peculiarities of *grandiflora:* (1) Light pubescence of stems, buds, hypanthium, ovary (and capsules); (2) Leaves lanceolate, plane and petioled; (3) Long buds with slender cones, and sepal tips much attenuate; (4) Long and delicate hypanthia. I have no hesitation in placing these specimens among the narrower-leaved forms of *Oenothera grandiflora* Solander.

The history of the specimens is not known at the Muséum d'Histoire Naturelle, but de Vries (1901, footnote to p. 317) suggests that they were collected by Abbé Pourret in the garden of the museum at the time of a visit to Paris in 1788. Since *grandiflora* was introduced into England and there grown as early as 1778 it is quite believable that as an American novelty, it might have reached Paris ten years later for striking novelties were frequently passed from one garden center to another.

6. Description of a Particularly Fine Specimen of *Oenothera grandiflora* Grown at Paris in 1815, from the Herbarium of Jacques Gay.

Plate IV

This remarkably well preserved specimen of *Oenothera grandiflora* Solander is in the General Herbarium of the Royal Botanic Gardens, Kew. It is from the herbarium of Jacques Gay purchased by Sir Joseph Hooker and presented to Kew.

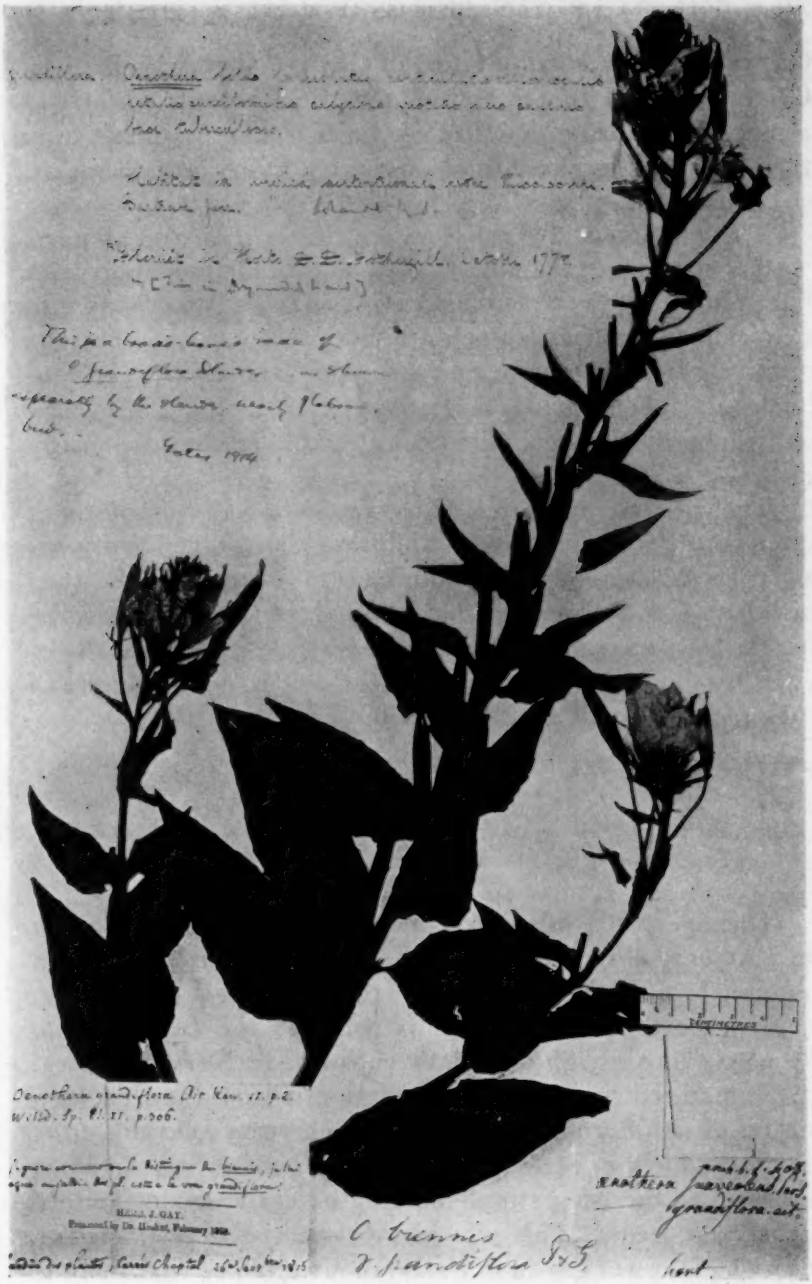

PLATE IV.—A well preserved specimen of *Oenothera grandiflora* Solander grown in the Jardin des Plantes, Paris, in 1815, about seventeen years after Lamarck wrote his description from material then in cultivation.

On the label in the handwriting of Gay is "Jardin des Plantes, Carrés Chaptal, 26 Septembre, 1815." Carrés Chaptal was a plot in the Jardin des Plantes, Paris, taking its name from the official who granted funds necessary for its establishment. (Bernard, 1842, p. 228).

The characters of *grandiflora* are clearly shown on all parts of this specimen (Plate IV). (1) The branching stem with light pubescence and petioled leaves. (2) The long buds with slender bud cones and much attenuated sepal tips. (3) The relatively light pubescence on sepal tips, and almost glabrous bud cones. (4) The long delicate hypanthia. (5) The ovaries with light pubescence, developing into long capsules nearly glabrous.

The specimen indicates that *grandiflora* was still a form of interest in Paris, as a showy exhibition plant, seventeen years after it was described by Lamarck (?1798) from material then in cultivation. That it should be an attraction is not surprising for the specimen is from a broad-leaved race obviously of vigor as shown by the size of leaves, buds and capsules and such races give tall plants which flower in profusion.

7. Discussion of Specimens from the Herbarium of André Michaux's Held by de Vries to be *Lamarckiana*. Plate V

These very interesting specimens (Plate V) in the "Herbier de l'Amérique septentrionale d'André Michaux" has been brought forward by Blaringhem (1914) and de Vries (1914*a*) as evidence that André Michaux collected the *Lamarckiana*, known to us through de Vries's cultures, in North America. If the material proved their contention it would indeed be a matter of significance. Michaux for ten years following 1785 traveled widely in eastern and southern United States and as far west as the Mississippi, making extensive collections of plants and seeds. This was just before the time when Lamarck's plant was grown in Paris (about 1796) so that there is a correspondence of dates.

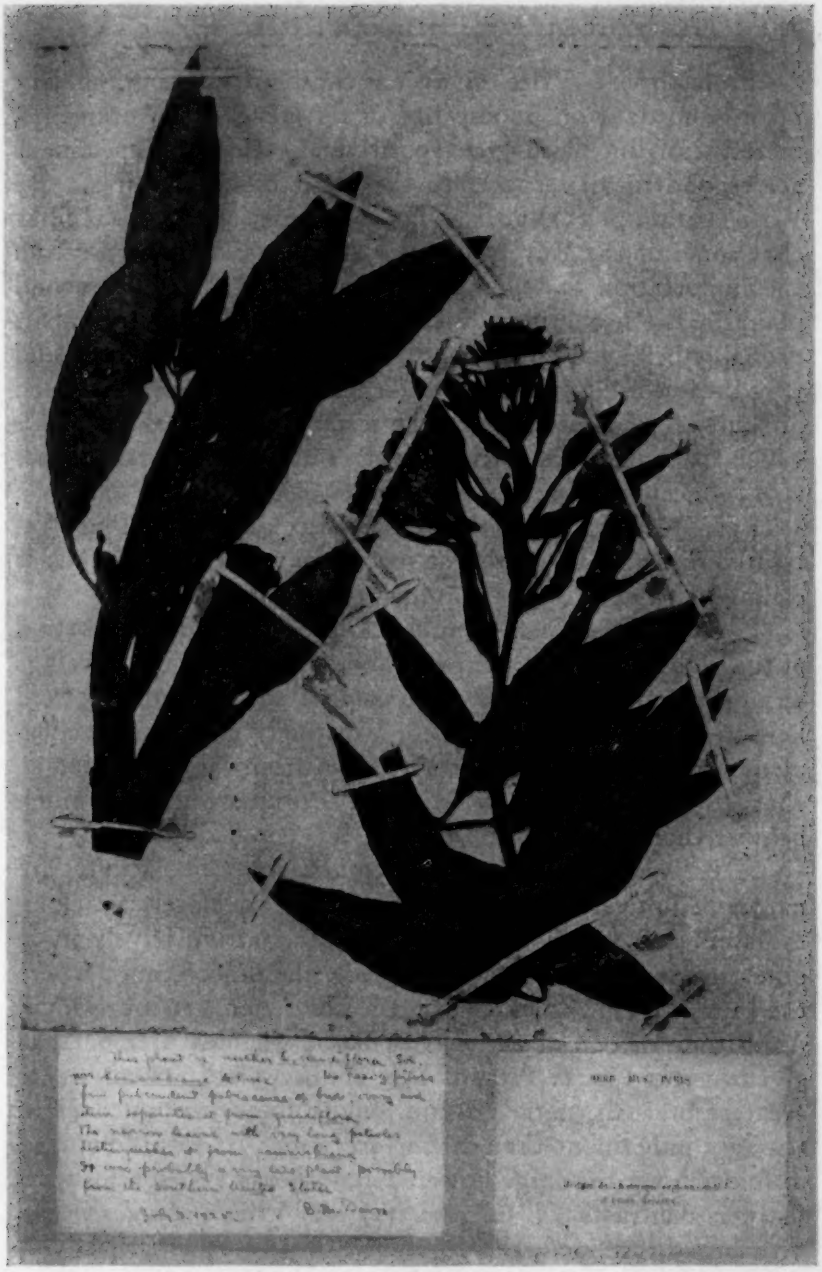

PLATE V.—Specimens collected by André Michaux in North America; held by de Vries to be *Lamarckiana*. It is uncertain what may be the affinities of this plant, but the characters of its remarkable foliage, together with other points, make it impossible to associate the specimens of Michaux's with the *Lamarckiana* of de Vries. The plant has no affinities with *Oenothera grandiflora* Solander.

Professor de Vries gives an excellent photograph but records no data on pubescence or on critical measurements, merely insisting with great positiveness that the specimens correspond in all respects to the plant of his cultures. De Vries (1914*a*, p. 357) goes so far as to suggest the probability that some of the European lines of *Lamarckiana* came from the importation of seeds by Michaux, and that the second plant in the herbarium of Lamarck, Plate II of this paper, probably belonged to this race. It is very hard to understand why Professor de Vries should put himself in a position of such difficulty for he has assumed the responsibility of evidence for which the material of Michaux's gives him no support. Gates (1915) accepts in full the conclusions of de Vries.

I give below a description of Michaux's material (Plate V), treated in detail as worthy of specimens for which so much is claimed and upon which so much of speculation rests.

1. *Stem.*—The sheet is unusual in showing a lower portion of the plant where the stem was about 18 mm. thick as well as an upper flowering shoot. The color is now the brown of old herbarium material. The stem is strongly pilose and puberulent but the long hairs do not arise from large papillae so characteristic of *Lamarckiana*. The pubescence is much heavier than that of *Lamarckiana*.

2. *Foliage.*—The foliage is remarkable. The leaves are narrow, elliptical and long-petioled; they are closely set along the stem and, as shown in Plate V, there is relatively little difference in size between those below the inflorescence where the stem is 8 mm. thick and on the older stem, 18 mm. thick. An outline of one of the leaves, based on measurements, is given in text Fig. 1. It was 15.5 cm. long and 2.8 cm. at its greatest breadth, petiole 2 cm. long, pubescence much more strongly puberulent than in *Lamarckiana*. It would be hard to find among the large-flowered oenotheras two species with sharper contrasts of foliage: (1) de Vries's *Lamarckiana* with broad, crinkled, short-petioled or almost sessile leaves, set relatively far apart along the stem and presenting a very wide range in size from basal leaves, 25–30 cm. long and 5 cm.

wide to the short leaves below the inflorescence; (2) Michaux's plant with narrow, plane, long-petioled leaves, closely set along the stem and with little range of size. The foliage alone would remove Michaux's plant from close affinities with *Lamarckiana*.

3. *Inflorescence.*—The inflorescence shows unusually large buds and flowers in the axils of very long and narrow bracts, much narrower than those of *Lamarckiana*.

4. *Buds.*—Buds at least 9 cm. long, cones stout, sepal tips thick and about 8 mm. long. These points are shown in the outline constructed from measurements in text Fig. 1. The sepal tips are strongly pilose and puberulent and the bud cone bears long hairs in a light puberulence. The hypanthium is pilose and puberulent. The form and pubescence of the bud is similar to that of *Lamarckiana*, but in size the bud is larger.

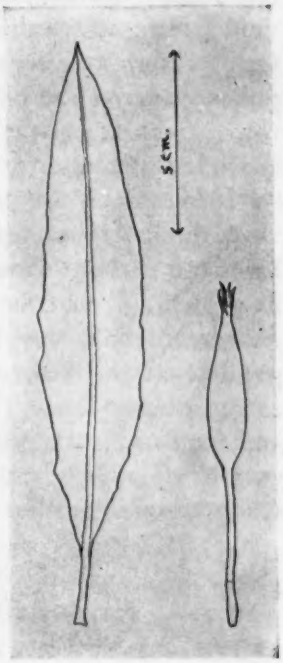

FIG. 1.—Leaf and bud of Michaux's plant (Plate V.) drawn from measurements.

5. *Flowers.*—The flowers are very large in keeping with the size of the buds; probably larger than in *Lamarckiana* and *grandiflora*.

6. *Ovary.*—The ovary bears a heavy pilose and puberulent pubescence as in *Lamarckiana*.

7. *Capsules.*—Unfortunately there are no capsules; in pubescence they must have been similar to *Lamarckiana*, we can judge nothing of their size.

I am not willing to make a guess as to the affinities of Michaux's plant. It is not *Lamarckiana* as shown by the remarkable and distinctive foliage, unusual among the oenotheras because the leaves are so narrow, so long-petioled, and relatively so small and closely set over what must have

been a large plant as indicated by the thickness of the stem. The narrow bracts, great length of buds, and absence of marked papillae, together with the heavier pubescence are also important differences from *Lamarckiana*. The plant obviously has no affinities with *grandiflora* from which the material differs at almost all points,—leaf form, bud form and pubescence over all of the plant. It is exactly on these points that Michaux's plant differs from both the first and second specimen of Lamarck's (Plates I and II).

It seems to me probable that Michaux's plant was tall with thick strong stems; narrow, petioled leaves, similar in form and without wide range of size; a plant coarse and weedy in growth in contrast to the symmetry of the large-leaved *Lamarckiana;* a very heavy pubescence without conspicuous papillae at the base of the long hairs; buds and flowers of extraordinary size. There are types of Oenothera in the south and west of North America that have these characteristics in various degrees but they have not as yet been given careful study through garden cultures.

8. A Specimen of Michaux's, Possibly *Oenothera suaveolens* Desfontaines

Professor de Vries in "Die Mutationstheorie" (1901, footnote to p. 316) referred to a specimen of Michaux's which is in the herbarium of the Muséum d'Histoire Naturelle, at that time believing the material to be *Oenothera grandiflora*. Further studies have led de Vries (1914*a*, p. 355) to the conclusion that it is the authentic specimen of *Oenothera suaveolens* Desfontaines. A photograph of this sheet has been published (Davis, 1912, plate 39). It holds two specimens and the larger is that under consideration. The sheet bears a label of Michaux's with "Ameriq. sept." On this label in the handwriting of Desfontaines is "*Oenothera suaveolens* Hort. Paris." Above this name has been written "*Oenothera grandiflora* Poiret Encycl," and below, Spach wrote: "*Onagra vulgaris grandiflora* Spach." A second label bears the name "*Oenothera grandiflora*" in the handwriting of André Michaux.

The specimens appear to have been collected by Michaux in America.

The larger specimen has a single mutilated bud with heavy pilose and puberulent pubescence; a capsule, 3.5 cm. long, also is strongly pilose and puberulent; the leaves are large and well petioled. The pubescence makes it impossible that the plant could have been *grandiflora*. The long capsule and petioled leaves remove it from *Lamarckiana*. It agrees in pubescence and length of capsules with the plant called *Oenothera suaveolens* Desfontaines which grows wild at various known stations in France. The smaller specimen on the sheet shows no characters sufficiently well to make safe an opinion as to its affinities.

Professor de Vries (1914*b*) and Blaringhem (1914) have made most important contributions in clearing some of the uncertainties concerning the status of *suaveolens*. I took occasion to visit in June of 1925 the village of Samois near Fontainebleau which is one of the stations studied by these authors and can confirm their conclusions. As de Vries makes clear *Oenothera suaveolens* is a well defined species with no close relationship to *grandiflora*. Its affinities seem most close to *Oenothera biennis* Linnaeus from which it differs conspicuously in greater height, larger buds, larger flowers, and much larger capsules. Possibly *suaveolens* had a southern range in North America with *biennis* more northerly. This material of Michaux's cannot be claimed as the type specimen, which is not known, but it does appear that Desfontaines considered the specimen to be *O. suaveolens* as cultivated in the garden at Paris.

9. The Probable Origin of *Oenothera Lamarckiana* of de Vries

The chief purpose of this paper has been to show that there is no evidence that the species, *Oenothera Lamarckiana* of de Vries, was known to Lamarck and that de Vries's determinations as his *Lamarckiana* of other material at Paris of the same period are also not correct. The herbarium of the Muséum

d'Histoire Naturelle in my opinion has no material of de Vries's *Lamarckiana* of dates earlier than the time, about 1860, when records of its presence in England are clear.

Professor de Vries (1914*a*, p. 358) and Gates (1915) in their concern to establish an early appearance of de Vries's plant in Europe have believed that the conspicuous Oenothera flora reported by Smith in the "English Botany" of 1806 as covering a large area of sand hills in Lancashire north of Liverpool contained *Lamarckiana*. I have published (Davis, 1926, p. 351–355) on an examination of specimens in Smith's Herbarium of the Linnean Society of London from "sand hills on the coast a few miles north of Liverpool, September, 1805." Smith's (1806) description in the "English Botany" was based on this material and Sowerby drew the figures illustrating the description from this or similar material.

The evidence that Sowerby worked from this material is furnished by colored drawings by Sowerby in the collections of the British Museum of Natural History [1] accompanied by the following note "Sent to Sowerby by Smith from Liverpool." It was the habit of Smith to preserve specimens of material on which illustrations for the "English Botany" were based. I have reported on these specimens in detail (Davis, 1926, p. 352–355, Plate II); they are unquestionably *Oenothera biennis* Linnaeus as shown by the smaller flowers, the stigmas below the tips of the anthers, and the pubescence. The English botanists have been correct in their opinion, generally held, that the Oenothera flora on the sand hills north of Liverpool in 1806 was *Oenothera biennis*.

Furthermore, the results of my examination of herbarium material in England (Davis, 1926, p. 355–358) show a record of collections of *biennis* by several English botanists from Lancashire sand hills before 1840 and frequent collections from the same regions from 1872 to recent dates. It is clear that *Oenothera biennis* since 1806 has been on the sand hills of Lancashire established as a wild species. This region has been

[1] In my paper of 1926, p. 353, I incorrectly stated that these drawings of Sowerby's at the British Museum of Natural History were in the collections of the Linnean Society.

an area of botanical interest and observation not only because of Smith's report in the "English Botany" but also because it has harbored a number of other foreign species probably introduced in part through the extensive shipping entering Liverpool and neighboring ports.

No collector reported *Lamarckiana* from this region until Bailey (1907) wrote of extensive colonies of the plant at St. Anne's-on-the Sea in Lancashire and distributed excellent herbarium material from this locality. Bailey states that he first gathered *Lamarckiana* along the estuary of the Ribble in 1881 which is the earliest record of its appearance in Lancashire. Records of *Lamarckiana* in England of earlier dates (for details see Davis, 1926, p. 366) are found in a specimen from the garden of Edward Leeds, 1876, from waste land at Banbury (Oxford), 1872, from waste land at Corkfield (Suffolk), 1871, and in the remarkable specimens grown by Dr. Asa Gray at Cambridge, Massachusetts, in 1862 from seed received from England (See Davis, 1926, p. 365, Pl. III).

For reasons given in my earlier paper (Davis, 1926, p. 367–371) I was unable to refer to *Lamarckiana* a number of specimens from English sources of narrow-leaved, large-flowered oenotheras with pubescence similar in character to that of *Lamarckiana*. These specimens are of dates from as early as 1827 to as late as 1907. If Michaux's plant (Plate V) had been grown in England it would have been included in this list. The material consists of a number of different forms with various and wholly unrelated histories.

Oenothera grandiflora has apparently never been widely cultivated in England and does not establish itself there as a garden escape for there are very few specimens in herbaria (Davis, 1926, p. 358–361), and statements of Sims's (1819) and of Wheldon's (1913, p. 207) indicate that the plant does not do well. *Oenothera grandiflora* to reach full development requires conditions of long, warm or even hot summers with abundant sunshine. *Oenothera biennis* on the contrary is vigorous and was the old evening primrose of cottage gardens. Now *Lamarckiana* has replaced *biennis* in favor and is a com-

mon plant of private gardens in south and central England against walls and in borders where it readily sows itself. The plant frequently escapes from cultivation and may be seen along railroad tracks in suburbs of cities. It seems probable that *Lamarckiana* established itself in Lancashire as a garden escape.

I cannot accept the views of Gates (1910, 1915) in his attempts to establish an early presence of *Lamarckiana* in Europe. With de Vries he has tried to associate with *Lamarckiana* early accounts of Oenothera floras, as that of Smiths's (1806) for Lancashire, and with certain early descriptions, and in my opinion he has failed to establish his contentions. His last claim that a sheet in the herbarium of Cambridge University with the writing "Oenothera grandiflora, Switzerland, J. Dalton" is the "exact counterpart of de Vries's race" has been discussed in my earlier paper (Davis, 1926, p. 372, Pl. IV., Fig. 2). These specimens, held to be of the early nineteenth century, have a pubescence of soft hairs scattered on stems and sepals, not at all the pubescence of *Lamarckiana*, and the bracts and leaves are too narrow for this species and the sepal tips are less attenuate.

In my opinion there is no date known for the appearance of *Oenothera Lamarckiana* of de Vries earlier than about 1860 when Carter and Company, seed firm of London, introduced the plant to the trade. Carter and Company referred the plant to the English botanist Lindley for identification and by his pronouncement it was given the name *Lamarckiana*. Professor de Vries (1914*a*, p. 359) lays great stress upon Lindley's opinion, but I cannot attach value to this identification in view of Lindley's obviously uncritical knowledge of *Oenothera* species as shown in his account of *Oenothera biennis* var. *grandiflora* (Lindley, 1833) with its confused synonymy. Even at the time Lindley's identification was questioned in correspondence published in "The Gardener's Chronicle," 1861, pp. 73, 98, and 122. I believe that Lindley picked the name *Lamarckiana* for the plant of Carter and Company on some principle of exclusion (since the plant was not of the species

then known in England) rather than on any personal knowledge of Lamarck's plant, which any comparison with the Paris specimens would show it not to be. The name *Lamarckiana* passed into the catalogues of the seedsmen and then through the studies of de Vries into genetical literature. In genetical literature it will certainly remain but it must be understood as *Oenothera Lamarckiana* de Vries and not the species of Seringe.

Carter and Company stated that the seed of their plant came from Texas, but American botanists have nowhere found the plant, except as a garden escape, and they have been well aware of the interest attached to the problem of its origin. Professor de Vries and Gates have expressed agreement with my views that the statement of a Texan origin is not correct. It has been not uncommon in past years for plants offered by seedsmen to be introduced with incorrect or loose statements of origin likely to arouse greater interest on the part of the purchaser, and several examples of this character are known to horticulturists. At times it has not been considered good business to admit a hybrid origin of cultivated plants offered for sale because of puritanical aversion on the part of purchasers towards "mongrels," and at times such material has been marketed as introductions from some foreign country.

It seems probable that Carter and Company, struck with the beauty and luxuriance of some garden plant or garden escape, obtained their *Oenothera* in England. We know from Lindley (1833) that large-flowered oenotheras were introduced and in cultivation in England as early as 1830. From specimens in the Lindley herbarium at Cambridge University, discussed in my earlier paper (Davis, 1926, pp. 368, 369), we know that some of these introductions from America were narrow-leaved species with pubescence similar to that of *Lamarckiana*; they were neither *biennis* nor *grandiflora* and the narrow leaves separated them from *Lamarckiana*. These introductions may have played a large part in forming Lindley's conception of his "*Oenothera biennis* var. *grandiflora*" which is figured as a large-flowered, narrow-leaved plant.

Of this Oenothera, Lindley (1833) writes "The plant now figured is not uncommon in gardens; it is one of the handsomest of all biennials and as easily managed as the common *Oenothera biennis* itself." I believe that out of this complex of large-flowered oenotheras, cultivated in England from 1830 on, the *Lamarckiana* of de Vries was segregated by Carter and Company and that there are abundant possibilities of its being of hybrid origin. The degree of success which I have attained in the synthesis of my hybrid, *Oenothera neo-Lamarckiana* (Davis, 1916, 1924), from a cross between *biennis* and the narrow-leaved western species, *franciscana*, is very suggestive of the hybridization that may have taken place among the oenotheras in gardens and waste lands of England, possibly resulting in the differentiation of that heterozygous species *Oenothera Lamarckiana* of de Vries.

The hope may still hold for some students of oenotheras that *Lamarckiana* will sometime be found as a wild species local to some part of North or South America. This cannot be ruled out as impossible, but I do not think it likely for the reason that the habit of *Lamarckiana*, the trimness and luxuriance of its growth, and the breadth and size of its leaves are very far from the aspect of wild oenotheras so characteristically weedy in appearance.

The question of a probable origin of *Lamarckiana* through hybridization is no longer a matter of such great concern to geneticists as might be suggested by the literature of past years. The genetical and cytological research upon this remarkable plant shows clearly that *Lamarckiana* is heterozygous, an impure species (Davis, 1922), with many complications introduced by the presence of lethals that prevent the full expression of the segregation that might be expected. The variants thrown by *Lamarckiana* are almost all to be explained by shifts of lethals permitting the expression of factors usually suppressed, or through numerical irregularities of chromosome distribution, as in non-disjunction responsible for a number of 15-chromosome types, and the chromosome behavior that determines the triploid and tetraploid forms.

These variants are of course the expression of segregation and are examples of Mendelian inheritance although very complicated in character. The variants described by de Vries as "mutations" from *Lamarckiana* seem to be all of this character. Only two gene mutations are held by Shull (1926, p. 204) to be fully established, these being the forms *funifolia* and *vetaurea*. As an impure species *Lamarckiana* must be regarded as behaving like a hybrid whether its heterozygous nature be in part the result of gene mutations, some at least of long standing, or whether it came about wholly or chiefly as the result of crossings.

It seems to me much more probable that the heterozygous nature of *Lamarckiana* is the result of hybridization since this process is known at times to bring on quickly complicated conditions of germinal constitution. Gene mutations in *Oenothera* seem to be rare or at least no more common than in animals and plants generally. The evolution of so involved a genotype as that of *Lamarckiana* through the summation of gene mutations would require an immensely long period of time, and it seems much more probable that hybridization has been the chief factor in developing its heterozygous constitution. However, as indicated above, it is no longer a matter of deep significance to geneticists when or where *Lamarckiana* arose. The performance of the plant under scrutiny of keen genetical and cytological analyses is showing to us an involved heterozygous genotype responsible for the very interesting peculiarities of its breeding behavior.

However, to students of oenotheras and to others it will always be a matter of interest to trace as far as possible the history of this remarkable plant, and this paper offers evidence that in my opinion removes *Lamarckiana* of de Vries from any connection with Lamarck's plant which is believed to have been a form of *Oenothera grandiflora* Solander.

Summary

Lamarck's description of his plant, published in the Encyclopédie Méthodique Botanique (?1798), is not in agree-

ment with the *Lamarckiana* of de Vries in the following important particulars: the capsules are described as smooth; the stem as almost glabrous, reddish brown, with numerous spreading branches; the sepals as ending in a short thread, bristle-like. The *Lamarckiana* of de Vries's in sharp contrast has hairy capsules; stems sparsely branched with a heavy pilose and puberulent pubescence, long hairs from red papillae over green portions of the stem; sepals not so attenuate.

Oenothera grandiflora Solander agrees with Lamarck's description in all respects except that the capsules are stated to be short. Since Lamarck in his description makes comparison directly with *Oenothera longiflora*, which has extraordinarily long capsules (about 5 cm.), there is good reason for believing that by "short" Lamarck meant short in comparison with *longiflora*.

There are two sheets in the Herbarium of Lamarck that may be associated with Lamarck's description. Both sheets bear matter in the handwriting of Lamarck that apparently relate either to his plant or to his description:—That on the first sheet (Plate I) "Aenothera . . . [grandiflora] . . . nova spec. flores magni lutei, odore grato, caulis 3 pedalis"; That on the second sheet (Plate II) "d'Amérique sept. Tige rameuse, haute de 3 à 4 pieds." Both specimens must have been known to Lamarck and both may have been concerned with his description which was based on dried material. Since the specimens on both sheets belong to the species *Oenothera grandiflora* Solander it is not a matter of great import whether one or the other or both be considered as the type.

I follow the opinion of Gagnepain and others of the staff in the Herbarium of the Muséum d'Histoire Naturelle and attach greater importance to the first specimen of Lamarck's (Plate I) partly because it bears the name "*Aenothera [grandiflora]*." There is no distinctive character of de Vries's *Lamarckiana* shown on this specimen, and it differs from *Lamarckiana* in pubescence of stem, sepals, hypanthium, and ovary, the branching of the stem, the distinctly petioled

leaves, more open inflorescence, slender bud cones with attenuate sepal tips, long and slender hypanthium. The specimen presents no character that is not in agreement with *Oenothera grandiflora* Solander.

The second specimen of Lamarck's (Plate II), held by de Vries to be the type of Lamarck's species, differs from the first specimen (Plate I) only in a slightly greater degree of pubescence over the stem and sepal tips, which, however, never resembles that of *Lamarckiana*, and in somewhat stouter bud cones such as are commonly found in oenotheras towards the end of a flowering season. It differs from de Vries's *Lamarckiana* in pubescence of stems, sepals, hypanthium, and ovary; the branching of the stem, the petioled leaves; attenuate sepal tips; long and delicate hypanthia. In no important respect does the second specimen differ from the first, and in agreement with the first specimen it is clearly of the species *Oenothera grandiflora* Solander.

Specimens on a sheet from the herbarium of Abbé Pourret (Plate III) believed by de Vries to be *Lamarckiana* are also *Oenothera grandiflora*. The specimens present no distinctive character of de Vries's *Lamarckiana* and show the following peculiarities of *grandiflora*: (1) light pubescence of stems, buds, hypanthium and ovary; (2) leaves lanceolate, plane and petioled; (3) long buds with slender cones and sepal tips much attenuate; (4) long and delicate hypanthia. I have no hesitation in placing these specimens among the narrow-leaved forms of *Oenothera grandiflora* Solander.

There is described and figured (Plate IV) a remarkably well preserved specimen of a broader-leaved race of *Oenothera grandiflora* Solander from the herbarium of Jacques Gay, in the General Herbarium of the Royal Botanic Gardens, Kew. This specimen is from a plant grown in the Jardin des Plantes, Paris, in 1815. It indicates that *grandiflora* was still a form of interest in Paris, as a showy exhibition plant, seventeen years after it was described by Lamarck (?1798) from material then in cultivation.

A very interesting specimen from the herbarium of André

Michaux's (Plate V), held by de Vries to be *Lamarckiana*, is neither *Lamarckiana* nor *grandiflora*. The specimens differ from de Vries's *Lamarckiana*: (1) in the remarkable foliage of narrow, long-petioled leaves, closely set on the stem and presenting little range in size; (2) in the very long and narrow bracts. The pubescence is in general similar to that of *Lamarckiana* but the long hairs on the stem do not arise from large papillae so characteristic of *Lamarckiana*. The specimens have obviously no affiliation with *grandiflora* from which the material differs at almost all points—leaf form, bud form, and pubescence over all of the plant. Michaux's plant was probably tall with thick, strong stems; narrow, petioled leaves, similar in form and without wide range in size; a plant coarse and weedy in growth in contrast to the symmetry of the large-leaved *Lamarckiana*; a very heavy pubescence without conspicuous papillae at the base of the long hairs; buds and flowers of extraordinary size.

A sheet holding two specimens of Michaux's from North America has been described and figured by Davis (1912, p. 529, Plate 39). The larger specimen is believed by de Vries to be the authentic specimen of *Oenothera suaveolens* Desfontaine. The long capsules and petioled leaves are not of *Lamarckiana*, and the pubescens makes it impossible that the plant could have been *grandiflora*. It agrees with *suaveolens* in pubescence and in the length of the capsule. I can confirm the important observations of de Vries (1914*b*) on the extensive stands of *suaveolens* about the village of Samois near Fontainebleau and I fully agree with de Vries that *suaveolens* has no close relationships to *grandiflora*. The affinities of *suaveolens* seem most close to *Oenothera biennis* Linnaeus from which it differs in greater height, larger buds, larger flowers, and much larger capsules. Possible *suaveolens* had a southern range in North America with *biennis* more northerly. This material of Michaux's cannot be claimed as the type specimen of *suaveolens*, which is not known, but it does appear that Desfontaines considered the specimen to be *suaveolens* as cultivated in the garden at Paris.

The probable origin of *Oenothera Lamarckiana* is discussed with reference to the identification of Lamarck's plant with *Oenothera grandiflora* Solander and the author's studies on the history of *biennis*, *grandiflora* and *Lamarckiana* in England (Davis, 1926). The following conclusions are brought out: (1) The herbarium of the Muséum d'Histoire Naturelle at Paris has no material of de Vries's *Lamarckiana* of dates earlier than 1860 when records of its presence in England are clear. (2) The Oenothera flora of Lancashire in 1806, described by Smith in the "English Botany" was *biennis*, and a series of collections by later botanists show only *biennis* (Davis, 1926). *Lamarckiana* was not noted in Lancashire until reported by Bailey (1907) who made his first observations in 1881. (3) There are a few known specimens of *Lamarckiana* from other localities in England (Davis, 1926, p. 366), but none earlier than 1860. The earliest specimens of *Lamarckiana* are believed to be from material grown by Dr. Asa Gray at Cambridge, Massachusetts, in 1862 from seed received from England. (4) Evidence submitted by Gates in his attempts to establish an early presence of *Lamarckiana* in Europe is not considered satisfactory. (5) The earliest date known for the *Lamarckiana* of de Vries is about 1860 when Carter and Company introduced the plant to the trade. No value is attached to Lindley's identification of this material as *Oenothera Lamarckiana* Seringe. It is not believed that Carter and Company received seeds of their plant from Texas. (6) We know from Lindley (1833) that large-flowered oenotheras were in cultivation in England as early as 1830, and from specimens in the Lindley herbarium at Cambridge University (Davis, 1926, pp. 368, 369) that some of these were introductions from America. They were forms with the type of pubescence found in de Vries's *Lamarckiana* but differed from *Lamarckiana* conspicuously in their foliage of narrow leaves. They have some points of resemblance to certain forms of southern and western North America, which have not as yet been the subject of garden study. (7) It is believed that the plant segregated by Carter and Company and introduced to the trade as

Lamarckiana arose in England among the large-flowered cultivated oenotheras, or garden escapes, quite possibly as the result of hybridization. We know from genetical and cytological studies that the *Lamarckiana* of de Vries has a complex heterozygous constitution maintained in line cultures because *Lamarckiana* for the most part develops only such fertile gametes as will in conjugation reproduce the heterozygous parent.

My opinions of 1912 respecting the identification of Lamarck's Oenothera grown in the garden of Paris about 1796 have been strengthened by personal study of the material in the Herbarium of Lamarck. This plant described by Lamarck (?1798) under the name *Aenothera grandiflora* and renamed by Seringe (1828) *Oenothera Lamarckiana* was a form of *Oenothera grandiflora* Solander (1789). The name *Oenothera Lamarckiana* Seringe becomes a synonym of *Oenothera grandiflora* Solander (*Oenothera grandiflora* "Aiton"). The plant so widely known as *Oenothera Lamarckiana* through the studies of Professor de Vries is very different from Lamarck's specimens or from any other form of *Oenothera grandiflora.* The plant of de Vries's should be allowed to keep the name so well established through genetical literature, but it should be understood as *Oenothera Lamarckiana* de Vries.

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THE ORGANIC CONTENT OF THE WATER OF SMALL LAKES

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This paper is a preliminary report of results thus far reached in the study of waters in the lake district of northeastern Wisconsin. The district includes Vilas and Oneida counties; it extends over one or more rows of townships in adjacent counties and also into that part of Michigan which lies just to the north. Its area, roughly stated, is about 10,000 sq. km. (3700 sq. mi.) and it has 1200 or more bodies of water large enough to appear on a map whose scale is one half inch to the mile. The larger part of these are small lakelets or tarns in depressions of the glacial drift, but more than 600 of them exceed one kilometer in their greatest dimension. Trout Lake, the deepest and one of the largest lakes, lies in lat. 46° 03′ N., long. 89° 42′ W. It is somewhat to the north of the center of the district. The work of the Survey has been done from the State Forestry Headquarters, situated on this lake.

The country is covered with a thick layer of drift and the lakes are typical for such a region. They are small and relatively shallow. The largest (Vieux Desert) has an area of 19.3 sq. km. (7.5 sq. mi.) and Trout Lake is 35 m. deep (117 ft.). The region was densely forested, mainly with pine. This has been cut and the area is now covered with young second growth of trees. There is little farming and the lakes are not affected by drainage from farms or villages. Much of the soil in the area is sandy, or gravelly; derived from the older rocks.

The Survey has begun a study of the typical lakes of this

region, carried on with assistance from the United States Bureau of Fisheries and the University of Wisconsin. Each lake has been treated rather as a specimen from a large group than as an organism whose individual life history must be studied as completely as possible. Thus the Survey hopes to ascertain the characteristic resemblances and differences between bodies of water which exist under similar topographical and climatic conditions but which differ in many other particulars.

The water of these lakes is soft or at most only medium hard. The mean amount of fixed carbon dioxide found in 113 lakes is 12.1 milligrams per liter of water; the range is from 0.75 mg. to 35 mg. The percentile distribution is: 0.75–1.9 mg., 20 per cent.; 2.0–4.9 mg., 9 per cent.; 5.0–9.9 mg., 6 per cent.; 10–14.9 mg., 25 per cent.; 15–19.9 mg., 25 per cent.; 20–24.9 mg., 11 per cent.; above 25 mg., 4 per cent.

Many lakes have neither inlet nor outlet and these have the smallest amounts of fixed carbon dioxide. Lakes close to each other may differ extraordinarily in respect to hardness of water. The maximum amount of fixed carbon dioxide in these lakes is hardly more than one half as much as the average quantity in the lakes of southeastern Wisconsin, which range from 40 mg. per liter of water to 80 mg. or even 100 mg.

Methods.—The organic matter of the centrifuge plankton of these lakes was determined by a method already described (Juday, 1926, p. 299–304). From two liters to six liters of water were evaporated at a temperature of 75° C., in order to obtain material for determining the organic carbon and organic nitrogen. Both of these were determined by micro-chemical methods, each determination calling for 5 mg. to 15 mg. of the dry residue. The organic carbon was determined by a combustion apparatus modified from that of Pregl by Mr. L. T. Hallett, working under the direction of Professor George Kemmerer of the University of Wisconsin. An account of this apparatus has been published (Kemmerer and Hallett, 1927). In a similar way, special apparatus has been devised for the micro-kjeldahl determinations and for the micro-

determination of the evolved carbon dioxide. An account of these will soon be published.

So far as is known this study is the first to employ microchemical methods in limnology. The work could not have been carried out in any other way. During July and August, 1926, about 250 samples of dry residue were obtained from 72 different lakes; in many cases these represented series of samples extending throughout the depth of the lake. Work on this scale called for chemical methods which require only a minimal quantity of material.

Data.—The general data for this study consist of physical, chemical, and biological determinations made during the summers of 1925 and 1926 on 84 different lakes and lakelets of the central part of the District. Only a small part of these data is used in the present paper, since some 20 or more items from each lake were determined or are in process of study. The particular data now considered are (1) 521 determinations of the organic matter in the centrifuge plankton of these lakes; (2) the determination of the organic carbon from 345 samples of dry residue; (3) the determination of the organic nitrogen from the same specimens.

Observations were made on 39 of these lakes both in 1925 and 1926 and 15 other lakes were visited two or more times in the same season. Altogether, 145 series of samples of dry residue were secured. In 60 cases there was only a surface sample; in 42 cases there were surface and bottom samples; and in 43 cases a series of 3 to 7 samples was taken. The largest number of samples came from Trout Lake, from which 11 series were taken, aggregating 47 samples. No other lake exceeded 14 samples and 9 lakes, besides Trout, had 9 or more samples.

In this paper the organic content of the water is considered by lakes; the simple mean of each series represents the series, and the mean of the series represents the lake. It follows that some lakes are represented by one sample only and others by a mean from many samples. But this fact is of less consequence since it appears that the general character of a lake in respect

to organic content is not subject to great variation. The lake preserves its general character from year to year, subject only to variations of minor importance.

The Organic Carbon

Quantity.—The mean amount of dissolved [1] organic carbon found in the 84 lakes examined is 6.04 mg. per liter of water. The smallest amount found in a single determination is 1.19 mg. at 9 meters in Diamond Lake (depth 11.5 m.); the largest is 22.82 mg. in Lake Mary at the bottom (20 m.). The minimum average amount from a lake is also from Diamond Lake (1.40 mg.) and the largest from Lake Mary (18.22 mg.). The maximum observation is therefore nearly 20 times as great as the minimum, and the maximum lake has 13 times as much organic carbon as the minimum lake. About 68 per cent. of the lakes have more than 3.00 mg. and less than 7.00 mg. per liter of water and the distribution of the lakes within these limits is fairly uniform. Beyond 7.00 mg. the number of lakes declines abruptly; there are 17 lakes whose organic carbon is between 6 and 7 mg. and only two between 7 and 8 mg. Between 8 and 13 mg. there are 15 lakes, distributed from 2 to 4 under each milligram; then come one lake with 14.5 mg. and one with 18.2. There are two lakes whose organic carbon is between 1 and 2 mg. and 5 lakes between 2 and 3 mg. Altogether about 8 per cent. of the lakes have less than 3 mg.; nearly 70 per cent. come between 3 and 7 mg. and about 23 per cent. extend along the range from 7 to over 18 mg.

The seven lakes with the smallest amount of organic carbon are those with no inlet or outlet and with little or no marsh adjacent to them. Most of them are small, deep in proportion to their area, with very soft and clear water. Five of them range in area from 15 to 52 hectares; four are deep, up to 20 m. and one is shallow; one lake has 189 ha. of area and is 16 m. deep; one, Clear Lake, has 323 ha. with a depth of 26 m. The last is the largest known lake of this type.

[1] For a discussion of the term "dissolved" see Birge and Juday, 1926, p. 186.

At the other extreme are 10 lakes whose organic carbon exceeds 10 mg. per liter of water. They belong to two very different types: (1) lakes which receive large amounts of extractives from marsh; they are either bog-lakes, lying in the center of a bog, or lakes which receive a supply of water from more distant marshes. (2) The second type of lake with high organic carbon includes those whose depth is small in proportion to their area, so that the summer circulation of water extends nearly or quite to the bottom. They have a relatively large supply of fixed carbon dioxide. Their plankton is usually large in amount, while that of the bog-lakes is often small. In the bog-lakes the supply of organic carbon comes in large part from sources outside of the lake; in the lakes of the second type it is largely due to the activity of life within the lake and these may be called active lakes. It must be noted that not all shallow lakes with high fixed carbon dioxide have also high organic carbon.

Lake Mary (1.1 ha., 22.5 m.) has more than 18 mg. of organic carbon and is the extreme case of a lakelet receiving much carbon from outside sources. It has about 2 mg. per liter of water of organic matter in its centrifuge plankton. Mud Lake (49 ha., 3–4 m.) has 14.5 mg. of organic carbon and 10.9 mg. per liter of organic centrifuge plankton. It is the extreme case of a lake of the active type, deriving its organic matter mainly from within.

Distribution.—In 85 cases series of samples were taken from 44 different lakes. These numbered from 2 to 7 samples. When two were taken they were from surface and bottom; in case of a larger number they were distributed by depth, ordinarily from 3 to 5 m. apart. The result of these series is to show that there is no significant difference in the amount of organic carbon at different depths. There are variations, but these are usually small and irregular. It was expected that the bottom water would show a larger amount than the surface and this relation is ordinarily found. But in about 40 per cent. of the cases the surface water had more organic carbon than the bottom and in an additional number that

difference was no greater than between samples from intermediate depths. The following cases will show the general situation (Table I.):

TABLE I

DATA FROM LAKES OF NORTHEASTERN WISCONSIN

All chemical quantities are stated in milligrams per liter of water

LAKE	DEPTH METERS	TEM. ° C.	CO_2		O_2	DRY RESIDUE	ORG. C.	ORG. N.	PLANKTON	PH.
			Free	Fixed						
Trout, 1,683 ha., 35 m., Aug. 14, 1926.....	0	19.6°	2.20	19.2	8.32	56.6	3.54	0.283	0.87	7.6
	5	19.6	2.00	19.1	8.35	57.9	3.53	0.318	1.04	7.5
	10	18.2	3.00	19.2	7.92	58.9	3.53	0.367	0.94	7.4
	15	9.2	5.70	19.3	7.09	59.5	3.36	0.333	0.81	7.0
	20	7.3	7.40	19.4	6.50	63.9	3.32	0.423	0.82	6.9
	25	6.6	9.00	19.5	5.21	63.4	3.49	0.448	1.19	6.8
	32	6.2	12.9	19.7	0.78	68.6	3.55	0.523	1.32	6.6
Adelaide, 23 ha., 21 m., Aug. 21, 1926.....	0	18.7	2.10	3.20	7.72	33.7	8.29	0.459	1.75	6.6
	5	14.0	7.40	3.20	4.39	31.8	7.16	0.433	1.73	6.2
	10	6.4	12.7	3.80	0.16	30.9	6.09	0.434	0.96	6.0
	15	5.8	16.0	4.30	0.06	36.6	8.35	0.498	1.48	5.9
	20	5.5	19.5	5.50	0.00	39.2	7.67	0.482	1.44	5.9
Diamond, 31 ha., 11.5 m., July 29, 1926.....	0	22.0	2.30	1.11	6.89	11.9	1.29	0.152	0.90	6.0
	5	22.0	2.30	1.15	6.84	13.6	1.46	0.195	1.11	6.0
	9	21.9	2.30	1.05	6.85	11.2	1.19	0.151		5.8
	11.5	21.0	5.50	1.15	5.30	14.3	1.65	0.231	1.62	5.5
Mary, 1.1 ha., 22.5 m., Aug. 21, 1926.	0	18.4	3.20	2.60	7.65	54.4	17.4	0.672	2.49	6.2
	5	4.8	22.0	4.20	0.45	55.5	16.8	0.715	2.12	5.8
	10	4.2	20.5	5.00	0.00	57.6	16.1	0.811	1.69	5.9
	15	4.6	33.9	7.50	0.00	69.0	19.0	1.200	1.92	5.9
	20	4.8	50.7	10.9	0.00	81.5	22.8	1.310	1.35	5.9

Trout Lake is the largest and deepest of the lakes examined. It has clear water and high fixed carbon dioxide for the region; it carries oxygen through the deep water in summer; its plankton is small in amount. Adelaide and Mary are two small moraine lakes; very deep for their area; with little fixed carbon dioxide and plankton; with water deeply stained

by extractives from peat marshes; and with little or no oxygen below the epilimnion in summer. They have large supplies of organic carbon from marshes. Lake Mary, as its record shows, is a remarkable lakelet in many ways. Diamond Lake is an extreme case of a clear-water lake with no supply of organic carbon from without; with very soft water and very little plankton. The water of all of the lakes has an acid reaction, as is almost uniformly the case in the District.

The items listed divide naturally into two groups in their relation to depth. Temperature, oxygen, and free carbon dioxide show marked changes with depth, and the same statement would be true for free ammonia and for net plankton. Dry residue, fixed carbon dioxide, organic carbon, organic nitrogen, and centrifuge plankton show relatively little change with depth, though there is variation and in general an increase toward the bottom. These cases may be taken as illustrations of the general situation. In Trout Lake, determinations were made of organic carbon and nitrogen from four surface samples taken at considerable distances from each other on the same date. They showed differences of the same order as do samples from the same place at different depths. Trout Lake shows unusual regularity in the series of organic carbons; Lake Adelaide shows unusual irregularity, as also does Diamond Lake, so far as percentage of variation goes, but the absolute quantities are very small in the last case. The organic carbon in Trout Lake is small; it is minimal in Diamond Lake; Lake Adelaide has an unusually large supply; and that of Lake Mary is the largest recorded.

It follows from these facts that it is not necessary to secure samples from all depths of a lake in order to ascertain its general situation as regards organic carbon and nitrogen. A single sample tells the story; the large number adds to knowledge of details and gives increased accuracy. But a single sample puts the lake in its true place in its organic relations, just as a single mineral analysis tells the story of its inorganic relations.

This statement for the distribution of the organic carbon

in depth may also be made regarding its distribution in time. In 39 different lakes determinations were made of the organic carbon in the summers of 1925 and 1926 and in 15 cases two or more determinations were made in the same summer. In general, the relation between the amounts of carbon found in the different years was much like that between two samples from different depths. The character of the lake remains the same from year to year, with variations which are usually of minor importance. The correspondence is often surprisingly close as in Clear Crooked Lake, whose organic carbon in 1925 was 3.62 mg. per liter of water and 3.65 mg. in 1926. More often the difference is greater, as in Allequash Lake, which had 6.16 mg. and 6.71 mg. Rarely, the difference may be more marked, as in Webb Lake, 1.88 mg. and 2.47 mg.; or as in Lost Canoe Lake, 3.47 mg. and 4.65 mg. In no case did the annual difference much exceed one third of the smaller quantity and it was ordinarily much less. Since the range of the quantity of organic carbon is great and the number of cases now on record is small, there is no use in computing the average variation.

There is little doubt that a greater number of cases and a longer series of years will show greater variation both in depth and in time than those which we have recorded. It is also improbable that a lake goes through the year with the same amount of dissolved organic carbon that is found in July and August. But it is plain that the organic carbon is one of the relatively fixed items in the economy of the lake, like the fixed carbon dioxide; and not one of the variable items, like oxygen or plankton.

The Organic Nitrogen

The data for the organic nitrogen are the same as for the organic carbon.

Quantity.—The average quantity of organic nitrogen found in the waters of 84 lakes, using for each lake the mean of all observations, is 0.493 mg. per liter of water. The smallest amount found in a single observation is 0.188 mg. in Webb

Lake and the largest is 1.540 mg. in the surface of Mud Lake. The only other lake which had more than one milligram of organic nitrogen is Lake Mary, whose lower water exceeded that figure (p. 362). Of the 84 lakes 43, or more than 50 per cent., had a nitrogen content between 0.300 mg. and 0.500 mg., and the lakes were uniformly distributed within these limits. Eight lakes, or about 10 per cent., had less than 0.300 mg.; 31 lakes, or nearly 40 per cent., were distributed along from 0.500 mg. to 0.980 mg.; and one lake exceeded 1.00 mg.

The range of variation in organic nitrogen is therefore much less than in organic carbon, the maximum being hardly 8 times the minimum as compared with a range of 20 times in case of the carbon. There is no such abrupt drop in the number of lakes at 0.500 mg. as there is for the carbon at 7.00 mg. There were 23 lakes with organic nitrogen between 0.400 mg. and 0.500 mg. and 12 lakes between 0.500 mg. and 0.600 mg. The numbers went on up by tenths of a milligram with 9, 4, 3, 3, lakes in each class.

The lakes with less than 0.300 mg. are in general the same as those with less than 3.00 mg. of organic carbon. There is no such close relation between lakes with high carbon and high nitrogen. Only four of the 10 lakes with the highest carbon are in the list of the 10 lakes with the highest nitrogen. In general the lakes which derive large amounts of organic carbon from marshes secure a low percentage of nitrogen in these extractives.

Distribution.—In general the same statements may be made regarding the distribution of the organic nitrogen that were made regarding the organic carbon; but the amount of variation is greater and it is more regular. In a series of samples from different depths the amount of nitrogen changes more than that of the carbon, as may be seen from the data on p. 362. In 72 cases from 41 lakes the average nitrogen at the surface was 0.380 mg. per liter of water and at the bottom 0.481 mg.; while the average amount of organic carbon from the same samples was exactly the same at top and bottom. In the extreme case the nitrogen at the surface was 0.361 mg.

(Cardinal Bog) and at the bottom (5.5 m.) it was 0.995 mg. In the same sample the carbon was 7.58 mg. and 12.8 mg. respectively; and the centrifuge plankton was 1.18 mg. and 6.19 mg., the increase being mainly due to organic débris. In general therefore, the organic nitrogen shows an increase below the thermocline in summer; an increase which is greater as the bottom is approached.

The lakes show about the same amount of organic nitrogen from year to year. The lakes examined in both summers had an average of 0.395 mg. in 1925 and 0.413 mg. in 1926—an insignificant difference. In May, 1927, 26 lakes were visited whose nitrogen could be compared with that of the preceding summer. The average for the summer of 1926 was 0.481 mg. per liter of water and for the spring of 1927, 0.523 mg. No lake had twice as much in the spring as in the preceding summer; there were 6 cases where the difference exceeded 50 per cent., five of these being excess in the spring. The influx of water from melting snow and the spring rains should cause the spring conditions to differ from those of summer. It appears therefore that the lakes preserve their character as regards organic nitrogen much as they do with regard to organic carbon.

The Carbon-Nitrogen Ratio

Since the organic carbon and the organic nitrogen are both variable and since they do not necessarily vary in the same direction, the ratio between them is more variable than either quantity. The mean ratio derived from 84 lakes, is C/N = 12.3 The smallest ratio found gave 7.4 carbon to one nitrogen. This was in a small shallow lake with very soft water. The highest proportion of carbon came from two bog lakes, in which the carbon was 26 and 27 times the nitrogen, the only lakes in which the carbon is more than 20 times the nitrogen. About 15 per cent. of the lakes have a ratio smaller than 10 to 1, and these include the lakes with very soft water and practically no contributions of carbon from without. About 25 per cent. have between 10 and 11 times as much

carbon as nitrogen; nearly the same number are between 11 and 12; and the remainder are distributed along from 13 to 20.4. The large figures for carbon are due to the presence in the water of the extractive material from marsh.

Since the organic carbon increases only very slightly with depth or remains stationary and the nitrogen shows a definite rise below the thermocline, it follows that the carbon-nitrogen ratio decreases with depth. In 40 lakes of this type the carbon was 13.4 times the nitrogen at the surface and 11.0 at the bottom.

In all cases the proportion of carbon to nitrogen in the dissolved organic matter exceeds that in the plankton organisms. Analyses of such organisms, both plant and animal, from southern lakes of Wisconsin show that the carbon is from 5 to 5.5 times the nitrogen. In the centrifuge plankton from the same lakes the carbon was from 5 to 7 times the nitrogen. No analyses have been made on plankton from northern lakes but there is no reason to suppose that it differs materially from that already analysed, since the organisms are the same.

In only two cases of lake residues was the carbon less then 8 times the nitrogen; and in most cases the ratio was much higher. The extractives from marsh and peat must contain a relatively small percentage of nitrogen, as also do those from the higher plants of the lakes. The higher plants are, however, very few in these soft-water lakes. But those lakes whose water is deeply stained with extractives from peat marshes have a high proportion of carbon—18 or more times the nitrogen.

The Total Dissolved Organic Matter

The organic material whose carbon and nitrogen have been studied is no doubt a mixture of numerous and different substances. Any computation of its amount from the data at hand can be only a rough approximation; but even an approximation yields interesting results.

The crude protein is computed as 6.25 times the organic nitrogen; the carbon in the protein is placed at 53 per cent.;

this amount is subtracted from the total organic carbon; and the remainder of the carbon is computed as carbohydrate, on the basis of 45 per cent. of carbon. For a discussion of this computation, see Birge and Juday, 1926, p. 196.

Computed on this basis, the average amount of dissolved organic matter in the 84 lakes is 12.8 mg. per liter of water. The range is from 2.9 mg. in Diamond Lake to 30.5 mg. in Mud Lake and 39.6 mg. in Lake Mary. The distribution of the lakes between these limits is the same as that derived from the organic carbon, since the total organic matter has a nearly constant relation with the organic carbon, being about 2.1 or 2.2 times the carbon.

The percentage of crude protein in the total organic matter, thus computed, bears a regular relation to the carbon-nitrogen ratio. The mean percentage is 24.8, or substantially 25 per cent. The range is from 11 per cent. in bog lakes, where C/N equals 25 or more, to 41 per cent. in an active lake where C/N equals 7.4. About one half of the lakes are pretty evenly distributed along the range from 23 per cent. to 30 per cent. of crude protein; about 30 per cent. of the lakes have less than 23 per cent.; and 20 per cent. of them have from 31 to 41 per cent.

The Centrifuge Plankton

There are altogether 521 determinations of the organic matter of the centrifuge plankton, coming from 81 lakes. The smallest amount from a single observation is 0.44 mg. per liter of water in the outlet of Rice Lake. The largest is 10.88 mg. from Mud Lake. The mean of the 81 lakes, giving each lake an equal weight, is 2.02 mg. per liter of water. This mean is greatly modified by the 10 lakes whose plankton is more than 3.00 mg. per liter of water and whose average is 5.57 mg. The remaining 71 lakes average 1.53 mg., and this amount gives a better notion of average conditions in the District. No such difference would be made in the means for dissolved organic matter by eliminating the ten largest cases.

The organic matter of the plankton is between 1.00 and 1.99 mg. in about 55 per cent. of the lakes; in 15 per cent. it is

below 1.00 mg.; in 18 per cent. it is between 2.00 and 2.99 mg.; and in 12 per cent. it is from 3.00 to 7.00 mg. These lakes with great amount of plankton are of the shallow and active type. Bog lakes and those with large amounts of extractive carbon usually have relatively small quantities of plankton; as also do clear, deep lakes with small quantities of fixed carbon dioxide. There is, however, no definite relation between quantity of fixed carbon dioxide and of plankton. Trout Lake, with about 19 mg. of fixed carbon dioxide per liter is among the lakes with hardest water; its plankton, from 116 observations, is 0.97 mg., less than one half the mean of all lakes.

Distribution.—In the surface water the plankton is usually smaller in quantity than in deeper water; but there is no regular increase with depth in most lakes. The bottom catch is usually the largest but is smaller than the surface in about 25 per cent. of the cases. In about 20 per cent., the bottom water has from 2 to 7 times as much centrifuge plankton as the surface; in such cases there is much organic débris present.

These lakes were studied during July and August, a time when the plankton should be at the summer minimum; but so far as present knowledge goes, there is no great difference between spring and summer. The lakes have been visited in May of the years 1925, 1926, and 1927; and the average amount of centrifuge plankton from 48 visits to 32 different lakes was almost exactly the same as in the summer—1.72 mg. per liter of water.

In general, the quantity of centrifuge plankton in the several lakes is much the same from year to year, although it is much more variable than the dissolved organic matter. In Trout Lake the surface catch in 21 series was 0.88 mg. with an average departure from the mean of 0.22 mg. or 25 per cent.; the minimum was about one half of the maximum.

Relation Between Plankton and Dissolved Organic Matter

The sum of the organic matter in plankton and that in solution gives the total organic matter of the water. The mean amount is 14.6 mg. per liter; the smallest is 4.1 mg. in Diamond Lake (see p. 362); the largest 41.8 mg. in Lake Mary; and the lakes are distributed between these limits with little tendency to concentration at any point.

The organic matter of the plankton averages 14.1 per cent. of the total organic matter of the water—about one seventh. This mean is very close to that found for Lake Mendota (Birge and Juday, 1926, p. 200). There is great variation from the mean in both the southern and northern lakes. The smallest per cent. in the northern lakes is 1.8 and the largest is 41.6. The bog lakes with high extractive carbon and with small quantities of plankton have the smallest percentage and the largest percentages are found in two types of lakes. These are the active, shallow lakes, with large amounts of both dissolved organic matter and plankton; and the small clear soft-water lakes with small quantities of both substances. Two of the four lakes with the highest percentage of plankton belong to each of these groups. About one third of the lakes come between 1.8 per cent. and 10 per cent.; more than one half between 10 and 20 per cent.; the remainder exceed 20 per cent., only two lakes having more than 30 per cent.

General Considerations

The lakes of northeastern Wisconsin show great differences both in their inorganic content and the organic content of their water. The maximum amount of the carbon dioxide of the carbonates is more than 40 times the minimum, and the maximum organic content is 10 times the minimum. There is no definite quantitative relation between organic and inorganic content. Lakes with the lowest organic content have also little inorganic matter; but the terms of this statement can not be interchanged and there is no general relation between high inorganic content and large amounts of organic matter. On this subject see the table on p. 362.

The lakes derive their organic matter both from internal and external sources. Many lakes in the District are dependent wholly on internal sources and may be called *autotrophic*. They receive water directly from rain and from a very limited drainage which has passed through a sand filter. The sandy nature of the soil in much of the District is the occasion for the presence of many such lakes, which have a character of their own. These lakes must derive their dissolved organic matter mainly from plankton and from their own higher animals and plants. These last items must be of little importance, especially as the soft-water lakes do not have beds of higher plants such as are common in the hard water lakes of southern Wisconsin.

Most of the lakes derive organic matter both from within and from drainage bringing extractives from soil or marsh. Such lakes may be called *allotrophic*. In these small bodies of water the external sources may be of great importance and they often completely dominate the situation, as in the bog lakes.

The dissolved organic matter is a fairly definite quantity for each lake. It does not show great variation either with depth or with time. In this respect it resembles the mineral content of the water rather than the oxygen or the active plankton. The great changes in the oxygen content of the hypolimnion must produce correspondingly great changes in vital conditions; but these have surprisingly little effect on the total dissolved organic matter.

The organic matter of the lake consists of (1) living organisms, (2) organic débris, (3) colloids, (4) matter in solution. The centrifuge plankton includes the first item, most of the second, and some of the third. The dissolved organic matter includes most of the third item, all of the fourth and remainders of the first and second. In quantity the living organisms and débris are usually the smaller items.

Many characters of the dissolved organic matter, as seen in this study, recall those of the organic matter of the soil. It is fairly constant in quantity and composition, so that the

character of the lake may be known from a single determination; it greatly exceeds the actively growing crop of plankton; it is not readily decomposed beyond a certain point. It must persist with little change for a long time, somewhat like the humus of the soil.

There does not seem to be any common end-point which the average composition of this organic matter approaches in the course of its treatment in the several lakes. On the contrary, while the direction of the movement of change seems to be similar, each lake appears to preserve at all times and at all depths the general character of the mixture of substances with which it starts at the surface.

Superposed upon this relatively constant constituent of the lake is the active and variable living plankton, which recalls the field crop at once in its connections with the substratum and in its small quantitative relation to the total organic matter of the lake.

The apparent constancy and permanence of the dissolved organic matter may perhaps have some bearing on the question of its food value. But this question together with many others, remains for future study. At present we have only begun to ascertain certain elementary data for the study of the numerous and intricate problems offered by the internal economy of lakes.

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Wisconsin Geological and
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RECENT DISCOVERIES RELATING TO THE ORIGIN AND ANTIQUITY OF MAN [1]

By HENRY FAIRFIELD OSBORN

(*Read April 29, 1927*)

In the great drama of the prehistory of man converge all the many branches of science which have been cultivated and encouraged by the American Philosophical Society since its foundation two hundred years ago. In fact, we do not progress very far in this most difficult, as well as most noble, branch of biological research if we pursue pathways which are purely anthropological or purely archæological. It is such specialistic mode of attack which has led more than one generation of man into pitfalls of opinion and of theory from which there is no escape except by direct retreat. In the list of those who have been compelled to reverse engines are the names of many great anthropologists, among them the renowned Hans Virchow, the still more widely known Ernst Haeckel and, probably to your great surprise, no less a name than that of Thomas Henry Huxley. Virchow opposed the recognition of the Neanderthal skull of 1846 with pathologic and theologic preconceptions. Haeckel also eagerly espoused the Ape Ancestry hypothesis by ignoring the profound cleft between ape and man. Huxley failed disastrously in rating the Neanderthal man with recent types of man and threw Darwin completely off the track of this veritable missing link. Huxley, too, failed to visit the Foxhall quarry of Ipswich, site of the greatest discovery in modern times, namely, the fireplace and tool flint quarry of Tertiary man. Even Jupiter nods when the purely specialistic pathway is pursued.

[1] Address before the American Philosophical Society at Philadelphia, on April 28, 1927, on the occasion of the celebration of the two hundredth anniversary of the foundation of the society.

In the triumphs of modern astronomy, four sciences converge, namely, mathematics, mechanics, physics and chemistry; but in the triumphs of anthropology, beginning with its dawn in the mind of Blumenbach, 1796, and reaching a succession of climaxes in 1927, no less than twelve of the major and minor branches of science converge, as follows:[1] The astronomy of Croll (1875) and Wallace (1880); the glaciology of Geikie (1894–1914), of Penck and Brückner (1909), of Leverett (1910); the glaciology and river terraces of Depéret (1918–1921); the palæogeography of Suess (1885), of de Lamothe (1899–1918), of Daly (1920–1926); the clay laminæ of De Geer (1910–1921); the loess of Schumacher, of Merzbacher, of Obruchev, and of Soergel (1924–1927). A host of other lines of research conspire to portray the great successive phases in the environment of man.

These great stepping stones of the Age of Man, of the Quaternary, of the Glacial Period, lead to our modern and greatly extended conceptions of the antiquity of man. Whereas Charles Lyell, in his classic work, "The Antiquity of Man," postulated 400,000 years for the Quaternary Period, we have practically multiplied the Glacial Age of Agassiz by four in the demonstration that there were not one but *four* titanic glaciations during Quaternary time and have thereby reached a minimum estimate of 1,000,000 years for the Age of Man.

But to complete the human prehistoric panorama as it is now painted, we cannot stop with the inorganic sciences. It is necessary to muster the whole galaxy of organic sciences—botany, including palæobotany; zoölogy, including palæontology; anatomy, including comparative anatomy and embryology; anthropology, including ethnology and archæology. The latest of the biological sciences to make its tribute is psychology, including comparative and physiological psychology, especially, of late (Tilney, 1926–1927), the localization of functions in the brain, and finally, the latest of the

[1] Osborn-Reeds: "Old and New Standards of Pleistocene Division in Relation to the Prehistory of Man in Europe," pp. 413, 414.

psychic cluster, known as behaviorism. It is our recent studies of behaviorism of the anthropoid apes as contrasted with the behaviorism of the progenitors of man which compel us to separate the entire ape stock very widely from the human stock.

While these twelve or more branches which bear upon anthropology have been advanced chiefly through the brilliant researches of specialists, it is our privilege and opportunity on this bicentenary occasion to gather all the reins and endeavor to present a truly philosophical series of generalizations, which may be summed up in advance under four chief captions:

(1) The antiquity of man is now to be reckoned not in thousands, but in hundreds of thousands of years, and we foresee the soon approaching period when it will be reckoned in millions of years.

(2) The Age of Man, or Pleistocene, can no longer be regarded as Act I of the prehistoric human drama, but rather as the final act, because at the very beginning of the Pleistocene we find the human race well established and widely distributed over the earth. Act I of the Age of Man is during Tertiary time in what may be known as the "Dawn Man" stage and the "pro-human" stage.

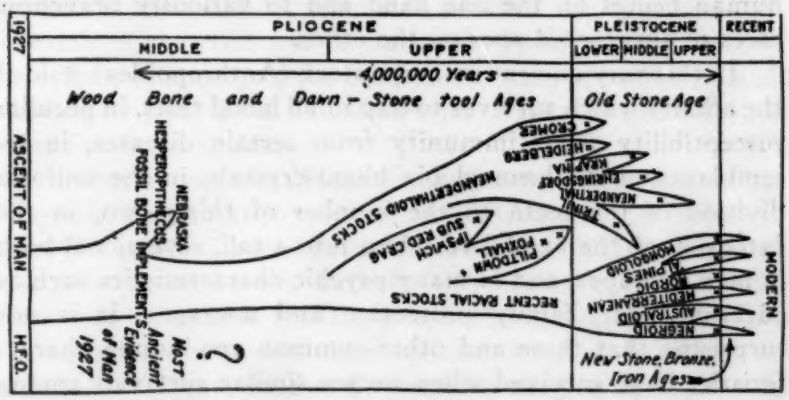

FIG. 1.—Prehistoric and recent racial stocks. Neanderthal stocks (left); partly known Pliocene stocks (center); six Pleistocene and recent racial stocks (right). (Below) Level of the supposed fossil bone implements of the Hesperopithecus quarries in Nebraska—possible evidence of Middle Pliocene bone-tool age in America.

(3) While still supported by very able anatomists such as Gregory, the ape-human ancestry theory is, in my opinion, greatly weakened by recent evidence, and I am inclined to advocate an independent line of Dawn Man ancestors, springing from an Oligocene neutral stock, which also gave rise independently to the anthropoid apes.

(4) The Dawn Man line belongs to a distinct family, the *Hominidæ*, ground-living, cursorial, alert, capable of tool-making, and living in a relatively open country on the high plateaus and plains of northern Asia.

(5) The Anthropoid Ape line belongs to a distinct family, the *Simiidæ*, tree living, brachiating, sluggish, incapable of tool making, restricted to the forests of south temperate and tropical countries.

Let Us Abandon the Ape-Human Theory

The prologue and the opening acts of the human drama occurred way back 16,000,000 years ago in the Upper Oligocene Period. At this period, or before, the family of man sprang from a stock neither human nor ape-like, but possessing certain common attributes which have been transmitted over this very long period of time to variously branching races of human beings on the one hand and to variously branching races of anthropoid apes on the other.

In this very ancient man-ape stock (Anthropoidea) resided the affinity which survives to-day in all blood tests, in peculiar susceptibility to or immunity from certain diseases, in resemblance of the hæmoglobin blood crystals, in the uniform division of the teeth to the number of thirty-two, in the extension of the caudal vertebræ into a tail, reversional both in man and apes, and in many psychic characteristics such as curiosity, fear, family protection and courage. It is not surprising that these and other common ape-human characteristics have survived when we see similar survivals among other animal stocks which we know parted company millions of years ago. Of all substances ever discovered, the heredity or the hereditary germ-plasm on which all these survivals

depend is the most stable. The germinal stability which has preserved the earliest Cambrian organisms over a period now estimated at 500,000,000 years is also capable of preserving pro-human anatomical and physiological traits for the relatively brief 16,000,000 years which have elapsed since the close of Oligocene time.

Consequently, many of the resemblances between ape and man which have been erroneously cited as proofs of ape-man descent are due either to very remote common inheritance or to the *convergence* of the ape toward the human type. An example of such convergence to the human type is shown in the foot of the gorilla by the recent observations of Akeley, of Morton and of Gregory. I regard the ape-human theory as totally false and misleading. It should be banished from our speculations and from our literature not on sentimental grounds but on purely scientific grounds and we should now resolutely set our faces toward the discovery of our actual pro-human ancestors. In my opinion, the most likely part of the world in which to discover these "Dawn Men," as we may now call them, is the high plateau region of Asia embraced within the great prominences of Chinese Turkestan, of Tibet and of Mongolia. The great plains area north of the high plateau should also be searched, because we have recently determined that this was probably the home of the primitive horse and, according to our theory, the home of primitive man should be looked for in the same kind of country in which the primitive horse flourished.

In abandoning the Haeckel ape theory which reached its apogee in the fantastic speculation of Klaatsch that different races of anthropoid apes gave rise directly to different races of man, we now give an entirely new frame to the human prototype to separate it sharply from the anthropoid ape type. Reconstructing our pro-human ancestors and endeavoring to assign an adequate date to the origin of the pro-human stock, we depend on the science of phylogeny, which has become in itself one of the finest products of human scientific endeavor. Phylogeny made a brave start in the sciences of comparative

anatomy and embryology but it awaited palæontology to place it upon a broad and firm foundation. Most of the recent advances in anthropology have been by palæontologic means and methods.

To build up the unknown human prototype by phylogenetic means we must take advantage of the really marvelous knowledge gained from all the minor and greater steps in the ancestry of the horse, of the rhinoceros, of the tapir and of the titanothere since these animals were first discovered in North America by the great Joseph Leidy, of Philadelphia, in 1856. Our pro-human ancestors, through their behavior, their tastes, their habits, and their fondness for travel, were the architects of their own destiny, as the horses and titanotheres were the unconscious architects of their destinies. Moreover, the open country best adapted to the evolution of the horse is also best adapted to the evolution of the higher races of man. We have determined that the horse did not evolve in southern Asia or even in the southerly portions of the high plateau regions of central Asia. To the North is a great unexplored plateau and plains region of Asia which now appears to have been the center of the origin of the family of horses and possibly may have been the center of the origin of the family of man. Certainly, the family of man could not have originated in a densely forested country rich in natural food materials. Man's nomadic wandering instinct, which even in Upper Pliocene time impelled his migrations, is not a forest characteristic but a characteristic of the open country. Almost without exception, precocious human civilizations have been found in open country partly deforested either by secular desiccation or by the severity of the northern steppe climates. Practically the same environmental conditions have favored the precocious development of the finer races of horses.

Secondly, when we at last discover one of our pro-human ancestors in Miocene or even in Oligocene time, the human characteristics will be found plainly stamped on this ancestor, as the horse characteristics are plainly stamped on the Pliohippus, on the Protohippus, on the Mesohippus and even

on the Eohippus. It was my observation of the full-bred horse of Middle Pliocene time, known as *Pliohippus leidyanus*, which led me to predict to the National Academy of Sciences the discovery of a full-brained pro-man also in Pliocene time; this prediction preceded the recent demonstration that *Eoanthropus dawsoni* of Piltdown is probably of Pliocene age.

This distinctive pro-human stamp will be seen chiefly in certain outstanding characteristics of habit and of structure which were acquired millions of years ago. In contrast with the Simian and pro-Simian stamp, we may clearly present in two columns the chief characteristics:

Hominidæ (Family of Man)	Simiidæ (Family of Apes)
Pro-human characteristics	*Pro-ape characteristics*
(1) Progressive intelligence, rapid development of the forebrain	(1) Arrested intelligence and brain size
(2) Ground-living bipedal habit—cursorial, adapted to rapid travel and migration over open country	(2) Arboreal to hyper-arboreal quadrumanal habit—living chiefly in trees
(3) Bipedal habit and development of the walking and running type of foot and big toe	(3) Quadrupedal habit when on the ground
(4) Shortening arms and lengthening legs	(4) Lengthening arms and diminishing legs
(5) Development of the tool-making thumb, fingers and hand	(5) Loss of the thumb and absence of tool-making power
(6) Walking and running power of the foot enhanced by enlargement of the big toe	(6) Grasping power of the big toe for climbing purposes, modified when walking

In the remarkable discoveries and studies of Boule, of Dubois, and of McGregor on the fossilized limb bones of man and in the complementary studies of Schultz in the embryogeny of man, the ape-arboreal-reminiscent hypothesis has not been strengthened; it has, on the contrary, been greatly weakened. The thigh-bone of the Neanderthaloid types resembles that of a man rather than that of an ape; it reveals the erect bipedal, rather than the stooping quadrupedal, position. The arms of the Neanderthals are not elongated as they should be according to the ape ancestry hypothesis; they are rather short. The legs of the Neanderthals and of the Trinils are not abbreviated as they should be for the ape

ancestry hypothesis; they are decidedly long. Similarly, a superb series of embryonic hands and feet of unborn infants assembled by Schultz do not reveal reminiscences of the attenuated ancestral fingers of an ape-arboreal stage, resembling those of gibbons, chimpanzees, or even of gorillas, but they are short and blunt like modern human hands. The embryonic thumb, similarly, is well developed and reveals no symptoms of recovery from the abbreviated or useless thumb characteristic of all arboreal or brachiating types of primates. Still more, the embryonic big toe, while slightly set apart from the other toes, shows little vestige of former limb-grasping such as is seen in the foot of the anthropoid apes, which is so hand-like as to give the bearer the title *quadrumana*, or four-handed. The human embryonic big toe is set apart like the toes of *Eocene* lemuroids such as the *Notharctus* of Leidy and Gregory.

Comparative and human psychology also weakens rather than strengthens the ape-man hypothesis. The geologic rearrangement of the Piltdown, the Trinil and the Heidelberg races which we owe to recent geologic discovery renders both the Heidelberg and the Piltdown races far more ancient than we had supposed. All the present evidence points to closing Pliocene age for the Piltdown Dawn Man, appropriately named *Eoanthropus* by Smith Woodward. This Dawn Man has a flat vertical forehead like the modern Bushman, a very thick skull, a chimpanzee-like jaw, and a surprising brain capacity of 1,240 cubic centimeters. This brain cube equals that of the existing Indian Veddah tribes. As analyzed by Elliot Smith and by Tilney, this Dawn Man has a well convoluted forebrain, speech areas and diversified motor areas for the coördinated motions of the fore limbs, of the hands and of the fingers.

Large Tertiary Brain Capacity

The Heidelberg race, now recognized as of Lower Pleistocene, is probably a giant pro-Neanderthal, characterized by projecting eyebrows and by a brain which would probably prove to be somewhat inferior in capacity to the more recent

Neanderthals. We consequently reach an entirely new estimate of the brain capacity of the human race at the close of Pliocene time and the beginning of Pleistocene time, a period estimated at between 1,250,000 to 1,600,000 years before our era.

BRAIN CUBE OF THE NEANDERTHAL AND TRINIL RACES

	MALE	FEMALE
Neanderthal Caveman of Western Europe:		
La Chapelle aux Saints	1,530	.....
La Quina, France	.....	1,367
Gibraltar, Spain	.....	1,280
Trinil Man of Java (*Pithecanthropus erectus*)	940	.....
Piltdown Man of Sussex, England	1,240	.....
[1] Native Indian Veddahs	1,012	1,037

The Trinil man of Java, *Pithecanthropus erectus* of Dubois, was formerly regarded as of Upper Pliocene, but now it is assigned by Dietrich and Osborn to a more recent geologic age, namely, Middle Pleistocene, since its fossil remains are found associated with stegodontine elephants much more recent in character than those of the Upper Pliocene, such as *Stegodon insignis ganesa.* Meanwhile, the brain of the Trinil man has been shown by Tilney to be distinctly pro-human, with a fairly well-developed forebrain or intelligence area. Consequently, we may now regard *Pithecanthropus erectus* as a very primitive type, a case of arrested development, possibly related to the Neanderthal stock, surviving in the southern sub-tropical forests of Asia, with a brain capacity of 940 ccm.—not far inferior to the minimum brain capacity of 1,012 ccm. of the native Indian Veddahs.

It required a very long antecedent period to develop the Dawn Man brain capacity and Dawn Man intelligence as demonstrated, in the case of the Piltdown and probably contemporaneous Foxhall races, in the manufacture of many different kinds of small flint implements and in the use of fire. In the case of the Heidelberg race, we observe the manufacture of very large and varied flint implements, such as are found at Cromer on the eastern coast of England and which are believed to be of the same geologic age as the Heidelberg jaw.

[1] Minimum brain cube.

Flint tools were, however, by no means the first tools employed by man; they were almost certainly preceded by bone tools of great variety, and bone tools were in turn preceded by wooden tools. Not improbably there was a very long "age of wood," then a very long "age of wood and bone," followed by a very long "age of flint" preceding the metal ages. During this enormously long period, which we must now reckon in millions of years, tool-designing and tool-making, the adaptation of tools to certain purposes and needs of life, the use of these tools in offense and defense, in the chase, and in the preparation of food and of clothing laid the foundations of the intelligence of mankind.

HOMINIDÆ (Family of Man)	SIMIIDÆ (Family of Apes)
Pro-human psychology and behavior	*Pro-ape psychology and behavior*
(1) Tool-making capacity of the hands and especially of the thumb	(1) Limb-grasping capacity of the hands and loss of the thumb
(2) Adaptation and design of implements of many kinds in wood, bone and stone	(2) Adaptation of the fore and hind limbs to the art of tree climbing and brachiating
(3) Design and invention directed by an intelligent forebrain	(3) Design limited to the construction of very primitive tree nests
(4) Use of the arms and tools in offense, defense and all the arts of life	(4) Use of the arms chiefly for tree-climbing purposes; secondarily for the prehension of food and grasping of the foe
(5) Use of the legs for walking, running, travel and escape from enemies	(5) Use of legs in tree-climbing and limb-grasping
(6) Escape from enemies by vigilance, flight and concealment	(6) Escape from enemies by retreat through branches of trees
(7) Tree-climbing by embracing the main trunk with the arms and limbs after the manner of the bear	(7) Tree-climbing always along branches, never by embracing the main limbs and trunk

The above are only a fraction of the host of psychic contrasts which might be drawn between the daily behavior of the Dawn Man and the daily behavior of the pro-anthropoid ape. As I have elsewhere summed it up, in the life and conduct of the pro-ape was the potency of the super-apes living to-day—the orang, chimpanzee, gorilla and gibbon—but in the Dawn Man was the potency of modern civilization. The most welcome gift from anthropology to humanity will be the banishment of the myth and bogie of ape-man ancestry and

the substitution of a long line of ancestors of our own at the dividing point which separates the terrestrial from the arboreal lines of primates.

It is true that Darwin used the expression, "Man is derived from some member of the Simiidæ," and that the term "ape-man" is deeply engraved in our consciousness, but I claim that it is misleading. The gorilla, chimpanzee and gibbon give us our conception of the ape. I hold that very few of the ape characters were possessed by man in his early stages; they are all characters belonging to an extremely

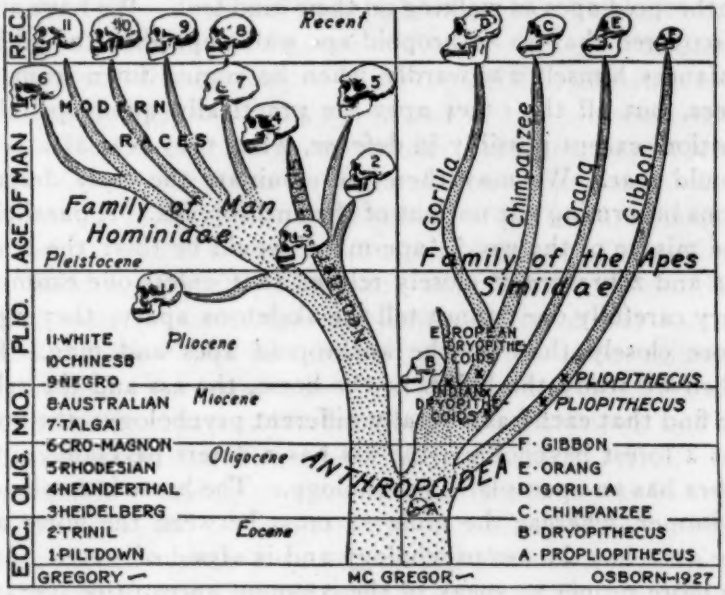

FIG. 2.—Recent evidence as to the ascent or phylogeny of man. (Left) Family of man, *Hominidæ*, dividing into the Neanderthaloid (right) and modern racial (left) stocks. Present geologic location of the Piltdown, Heidelberg, Trinil, Neanderthal and Rhodesian fossil races (left). (Right) Family of the apes, *Simiidæ*, including the Pliocene and Miocene Dryopithecoids nearest the ancestral stock of the *Anthropoidea;* also the lines leading to the gorilla, orang, chimpanzee and gibbon. *Anthropoidea*—the common Oligocene ancestors of the *Hominidæ* (left) and of the *Simiidæ* (right).

ancient arboreal stage, perhaps as ancient as Eocene time. Comparative anatomists find likenesses between apes and man by blood tests, osteology and morphology; these characters are strikingly pro-human, and anatomists have dwelt on them

to the exclusion of others not human. Between man and the ape—not only the hands and feet of the ape, but the ape as a whole, including its psychology—you will find more differences than resemblances. In brief, man has a bipedal, dexterous, wide-roaming psychology; the ape has a quadrupedal, brachiating, tree-living psychology.

The term "ape-man" has been forced into our language along a number of lines, and even the term "anthropoid" has come to lose its significance. "Ape-man" gained prestige through early explorers and travelers who represented the anthropoid apes as walking on their hind feet. We have since discovered that no anthropoid ape walks upright; the gibbon balances himself awkwardly when he comes down from the trees, but all the other apes are practically quadrupedal in motion, except possibly in defense, when they rear as a horse would rear. We may therefore eliminate the early descriptions in forming our notions of the anthropoids. A parallel to the misuse of the word "ape-man" would be this: the horse, ass and zebra are so closely related that unless one examines very carefully one cannot tell the skeletons apart; they agree more closely than do the anthropoid apes and man. But when we study the habits of the horse, the ass and the zebra we find that each has a totally different psychology: the horse has a forest psychology, the ass has a desert psychology, the zebra has an open-plains psychology. The horse is a splendid swimmer, whereas the mule—a cross between the horse and the ass—has the ass psychology and is afraid of water. It is no more proper to speak of the common ancestor of the apes and of man as "ape-man" than it is to call the common ancestor of the horse and the ass an "ass-horse." Another instance of wide psychic difference between like animals is that of the black and the white rhinoceros of Africa, which have a very dissimilar psychology and react differently in every emergency.

Empire of the Low-Browed Neanderthal Races

We may class together as Neanderthaloid all the prehistoric races with prominently projecting supraorbital processes; with low, retreating foreheads; with correspondingly low, broad type of brain, especially with low forebrain in contrast with the relatively high forebrain of the Piltdown and of modern races; with massive jaw and retreating chin of the Heidelberg and true Neanderthal type. The increasing brain power of these Neanderthaloids during Pleistocene time is perhaps measured by contrast between the Trinil brain of 940 cubic centimeters and the most highly developed Neanderthal brain of 1,530 cubic centimeters. The psychology of this race is further revealed by the prevailing type of flint implement, of offense and defense, of the chase and in the preparation of food. The first of these great Neanderthal flint types is found in the Cromer deposits in East Anglia—tremendous flint implements used largely in combat. Over an enormously prolonged period these implements passed through Cromerian, pre-Chellean, Chellean, Acheulean and, finally, Mousterian stages, wherein they begin to show decadence and loss of virility, together with invasion of other types of implements.

The great Neanderthaloid race, with its characteristic stone culture, apparently dominated northern Africa and all of Europe and extended eastward into the heart of Asia. Its quarries and camping grounds increase in number as Pleistocene time goes on, and an eastward to southward spread may be represented in the recent discoveries of Mousterian camping sites in Ordos, China, and of a Neanderthaloid skull, which has been named the Rhodesian skull, at Broken Hill Mine, South Africa. The animal life contemporaneous with this race is well known; it included a large variety of elephants, chiefly of the southern and straight-tusked types, rhinoceroses and, in the lower lands, hippopotami. This is known as a South Temperate fauna adapted to rather fertile lands, river bottoms and abundant forests. In such an environment game was so plentiful that there was relatively little struggle for existence, hence there was little incentive to the development of a

diversified flint industry. Superior intelligence was not demanded and it is therefore surprising that under these circumstances the Neanderthal brain attained the dimensions which threw even the genius of Huxley off the track as to the very primitive character of this race. Taken altogether, the widely extending range of the Neanderthaloid races is one of the most firmly established facts of prehistory. If our geologic time scale is reliable, it extended over a period of 900,000 years, and if our present records of quarry grounds and implements are reliable, the Neanderthals had almost exclusive possession of an enormous territory.

Theory of the North Eurasiatic Origin of the High-Browed Races

It was formerly believed by certain anthropologists that the Neanderthals were the progenitors of the succeeding higher races, but in my opinion we may entirely abandon this theory and substitute a theory of the complete replacement of the Neanderthal empire by invading races that had acquired superior intelligence under entirely different conditions of life. In other words, while the Neanderthals were enjoying exclusive possession of central Europe, Asia and a large part of northern Africa and were spreading southward into Rhodesia, the progenitors of all the modern races were occupying another great area, under conditions of life in which the struggle for existence was much more severe and in which there were far greater demands upon the native wit of man to overcome natural difficulties by invention and resourcefulness.

This unexplored territory, the unknown homeland of the higher races of man, cannot be south of the Neanderthal Eurasiatic belt, because to the south conditions of life were less rigorous, food was more easily obtained, and the milder subtropical climate was less stimulating to discovery and invention. In this southern, less stimulating region of Eurasia may have survived the persistent Trinil race of Java and other primitive races still undiscovered. To the south, in Africa, may also have developed Negroid stock under central African conditions of life that must closely parallel those of central and southern Eurasia during the great Neanderthal period.

Consequently, it is to the northern regions of Eurasia that we must look for the unknown homeland of the higher races, to a temperate and north temperate region which extended along the northern borders of the Neanderthal empire over the high central plateau region of Asia, over the great plains region to the north of the central plateau and, finally, over the confines of eastern Europe. It may be laid down as a fixed principle in the rise of the intelligence of man that only when the struggle for existence is fairly keen does any race progress; when the struggle for existence is too severe the entire life is devoted to physical support, to the exclusion of intellectual and social progress.

The new modern races, pure and blended—Mediterranean long-heads, Alpine broad-heads, blended Cro-Magnons, Nordics—apparently moved eastward over this northerly plateau and plains region and finally subdued the entire central Eurasiatic empire of the Neanderthals. These new races were not only distinguished by large brains and by equal powers of observation, of reasoning, of design, of tool-making, and of social, moral and political organization, but were also endowed with higher intellectual, spiritual and creative faculties which gave them both physical and intellectual supremacy over the Neanderthals and led to their entire occupation of western Europe. First, to measure their capacity purely by the cube of their brain, let us place these newly arrived races in order of brain power with the existing races, high and low (*fossil races*):

	Cube of Brain Capacity		
	Male	Female	Maximum
Cro-Magnon	1,550–1,590		
Average modern European	1,450	1,300	
Average modern Swiss	1,467 (1,200–1,660)	1,349 (1,230–1,510)	1,660
Upper Palæolithic broad-head race of Ofnet	1,400		
Living broad-head race of Czechoslovakia	1,415 (1,230–1,800)	1,266 (1,000–1,400)	1,800
Native Australian race	1,310	1,154	
Native Indian Veddahs	1,250 (1,012–1,408)	1,139 (1,037–1,217)	1,408
Papuans of New Guinea	1,236	1,125	

The Cro-Magnons, who have been termed the Palæolithic Greeks, rank extremely high in their cubic brain capacity; they entered Europe side by side with the pure long-headed and broad-headed races, also of high brain power, and are now considered to represent a blend between long-heads and broad-heads. Imagine the enormously long period of time during which this very high modern brain power developed and consider through the astonishing industry and diversified art of these people that every faculty has its cerebral equivalent and ancestry for each of its several coefficients. The extreme accuracy of observation of animal form displayed by the Cro-Magnons is not the result of hundreds of years, but of hundreds of thousands of years.

It is possible that the Piltdown race of Upper Pliocene time with its 1,240 ccm. brain cube is an offshoot of the precociously large-brained stock that gave rise to the group of modern races—Australoid, Negroid, Mongoloid, Caucasian. Yet the Piltdown race has a chimpanzee or anthropoid ape type of jaw. It seems a very hazardous prediction, but I am inclined to anticipate the discovery, even in Pliocene time, of a modernized type of jaw with prominent chin. This is against all existing evidence, with the exception of the dubious Foxhall jaw with its prominent chin, for all the known Pliocene and early Pleistocene races have a sloping chin or less remotely resemble the anthropoid ape type.

Conclusion

Let us therefore conclude with consideration of the ancestry of man according to the modified concept of "dawn men," not "ape-men." In the first place, over an incredibly long period of time the Dawn Men have been tool-makers, of high adaptability and wonderful technique. We have then a biped, a being with a hand capable of grasping and controlling tools, a tool-maker with as fine a sense of touch as that of any of the present-day etchers, engravers and artists. In my opinion, the pro-man psychology, leaving out the evidence of anatomy and morphology, is certainly that of a Dawn Man

and not of an "ape-man." I agree with my colleagues that man passed through an arboreal stage, but I believe that this stage did not progress so far as to carry man into a stage approaching that of the anthropoid apes. Dollo has stated the law of the irreversibility of evolution. The brachiating hand of the ape was used as a hook—apes do not grasp a branch with the fingers and thumb but hook the whole hand over the branch, as trapeze workers do to-day—and the thumb was therefore a grave danger. If man had gone through a prolonged period of brachiating in the branches of trees he would have lost his thumb. I agree to putting our arboreal ancestors back to Eocene time, but I predict that even in Upper Oligocene time we shall find pro-men, and if we find Oligocene pro-man—in Mongolia, for example—that he will have pro-human limbs, not pro-anthropoid ape limbs.

Of all incomprehensible things in the universe man stands in the front rank, and of all incomprehensible things in man the supreme difficulty centers in the human brain, intelligence, memory, aspirations, and powers of discovery, research and the conquest of obstacles. The approach to this unknown field of future human advance—the seat of the human mind and the constitution of the human mind—is along the great paths of human and comparative anatomy and of human and comparative psychology and behavior. Yet this approach will yield only a tentative conclusion; the final solution of this problem of problems—the rise of man—will come only through unremitting exploration and the chance finding somewhere in the Eurasiatic continent of actual fossil remains of the Oligocene pro-man, of the Miocene and Pliocene Dawn Man and, finally, of the early Pleistocene ancestors of the large-brained modern races.

HENRY FAIRFIELD OSBORN,
Vice-President of the American Philosophical Society.

June, 1927.

THE SKULL OF EUSMILUS

By WILLIAM J. SINCLAIR and G. L. JEPSEN

(*Read April 30, 1927*)

Eusmilus is a European genus of sabre-tooth cats hitherto represented in North America by the right half only of a lower jaw, in the Princeton collection, from the Protoceras channel sandstones of the White River Oligocene, described in 1895 by Hatcher as *Eusmilus dakotensis*. The European material is almost equally meager, consisting, so far as yet published, of portions of lower jaws and upper canines from the Quercy Phosphorites, referred by Filhol, in 1873, to *Machærodus bidentatus* and again described in 1875, by Gervais, from similar remains, from the same source, as *Eusmilus perarmatus*. It was, therefore, an exciting moment, one day last August, when a skull of a large sabre-tooth cat, with associated right mandible, found by the junior author in the lower Oreodon beds, was recognized in the field as pertaining to an undescribed *Eusmilus* for which the name *sicarius* (the stabber) is now proposed. The locality where the skull was found is 8.5 miles south of Wall, Pennington County, South Dakota, in a great badland pocket, possibly draining into Crooked Creek. The stratigraphic level is the lower Oreodon beds of the White River Oligocene. The skull lay in a narrow gray band, two or three feet above the top of the main pinkish-gray stratum familiar to collectors as "the turtle-oreodon layer" or "red layer." The type specimen, consisting of the skull, of an old animal with associated right half of the lower jaw and fragments of the opposite ramus, Figs. 1–8, is No. 12953, Princeton University Geological Museum.

Dentition

The complete dental formula of *Eusmilus* can now be written for the first time: i. $\frac{3}{2}$, c. $\frac{1}{1}$, pm. $\frac{2}{1}$, m. $\frac{1}{1}$. The presence of two lower incisors was noted by Filhol [1] and is also characteristic of Hatcher's type of *E. dakotensis*.[2] Its recurrence in the newly discovered form is conclusive evidence of its generic significance. The lower median incisor in *E. sicarius* is represented only by the tip of its alveolus, *x*, Figs. 6 and 7, a rather small depression suggesting that this tooth was not as large as the lateral incisor, of which the crown had broken off and rolled down the bank below the skull. As now attached, this crown does not make perfect contact with the root remaining in the alveolus, but is probably correctly placed. It is recurved, with the internal lateral edge not as sharp as in *E. dakotensis* and the outer edge worn away by contact with the upper teeth.

The crown of the lower canine is broken off and was not recovered. To judge from the cross-section of root remaining, it had almost twice the anteroposterior diameter of the adjoining incisor and was remarkably flattened laterally, Figs. 2 and 6.

There were three upper incisors on either side, Fig. 5, but of these only i. 2 on the left side remained in place. An incisor crown adjoining it in the matrix has been interpreted as the left median. Three additional crowns, displaced in the soft clay preceding the more or less decayed premaxillary margin, are regarded as right i. 1–3, but their order is uncertain. The tooth crown inserted as left i. 3 had rolled down the badland slope beneath the skull which lay with its left side exposed. All the upper incisor crowns are pointed and downwardly curved.

The upper canines, with the exception of some fragments of the root of the right one still retained in the alveolus, Fig. 5, had decayed and been lost before the fossilization of the skull.

[1] *Recherches sur les Phosphorites du Quercy*, 1877, p. 153.
[2] *Am. Naturalist*, 1895, p. 1091.

Judging from the size and shape of their alveoli, they were teeth of immense size, very wide anteroposteriorly, extremely thin transversely and of great length, indicated by the depth of the jaw flange below which they probably projected when the mouth was closed. Their width, as restored in Fig. 2, is correctly indicated, but their length is an approximation only.

Apparently correlated with this remarkable upper canine development is the striking reduction of the effective shear of the cheek teeth, much more so than in *Smilodon* where the huge stabbing upper canines attain their maximum of length and lateral compresion. This dental specialization, seemingly in the direction of a stabbing rather than biting mode of attack, has suggested the specific name here proposed and correlates with various cranial structures presently to be mentioned. In the upper series, which is complete on the right side only, Fig. 1 *A*, *B*, there is but one really effective tooth, the sectorial premolar, which is preceded by a small pm. $\frac{3}{}$ and followed by an equally small, transversely placed m. $\frac{1}{}$ as in *Hoplophoneus*. The third upper premolar is like that of *Hoplophoneus* with a central cusp and two secondaries, the posterior larger than the anterior, but the crown is proportionately smaller and is much worn internally by contact with lower pm. $\frac{4}{}$ which also strikes against the inner

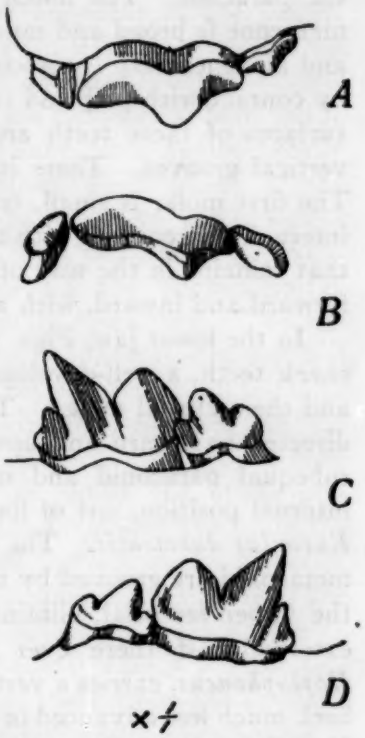

FIG. 1. — *Eusmilus sicarius*, sp. nov. *A*. Right upper cheek teeth from the outer side. *B*. The same, crown view. *C*. Right lower cheek teeth from the outer side. *D*. The same, from the inner side. All figures natural size, drawn from the specimen.

surface of the anterior blade of the upper sectorial, obliterating all trace of parastyle, if the latter ever was developed as a separate cusp, and forming an elongated shear anterior to the paracone. The notch between the latter cusp and the metacone is broad and much shallower than in *Hoplophoneus* and an inner cusp (protocone) is absent, perhaps worn away by contact with p. $\overline{4}$ and the lower sectorial, as the opposing surfaces of these teeth are scored by a number of shallow vertical grooves. There is, however, an inner root present. The first molar is small, transversely placed and much worn internally by contact with the rear of the lower sectorial. All that remains in the way of pattern is a cutting edge, convex forward and inward, with a single high central point.

In the lower jaw, Figs. 1 *C*, *D*; 2; 6; 7, there are but two cheek teeth, a well-developed double-rooted fourth premolar and the sectorial molar. The crown of the fourth premolar is directed backward and upward, with a high protoconid and subequal paraconid and metaconid, the former having an internal position, out of line with the other two cusps, as in *Eusmilus dakotensis*. The outer side of the protoconid and metaconid are grooved by contact with the paracone shear of the upper sectorial, eliminating all trace of basal cingulum externally, if there ever was one. The sectorial, as in *Hoplophoneus*, carries a vestigial metaconid and a still smaller heel, much less advanced in this respect than is the sectorial of *E. dakotensis* where metaconid and heel are lacking. The slight prominence noted by Hatcher on the posterior edge of the protoconid in *E. dakotensis*, shown in his Figs. 1 and 2 [1] and ascribed to wear of the opposing superior tooth, has certainly been accentuated by wear, but we are not certain that it is solely due to that cause. Filhol's figures [2] of *E. bidentatus* show a prominent convexity at this point, perhaps interpretable as a conjoined mataconid and heel. The entire outer face of the lower sectorial is more or less vertically grooved by contact with the opposing teeth of the upper series.

[1] *Loc. cit.*, p. 1092 and Plate XL.
[2] *Loc. cit.*, Figs. 140–142.

The Skull

Lying as it did, in soft clay, the skull suffered some lateral flattening, indeterminable in extent because symmetrical. Asymmetrical deformation is most apparent in the palate, along the median line, Fig. 5, where the left half overrides the opposite side, and also along the right maxillo-nasal suture. Farther forward, on this side, there is a rectangular bend in the right nasal, Fig. 3. The arches have been symmetrically concaved inward, toward each other, Figs. 3 and 5, the glenoid processes probably approximated to some extent with diminution in width of the brain case, and the basisphenoid groove and central keel on the supraoccipital, above the foramen magnum, certainly intensified. The left fronto-parietal area, including the postorbital process and temporal ridge, having weathered out first (the skull lay with the right side downward) had completely decayed and has been reconstructed in plaster, Fig. 3, from the opposite side. Various other parts are missing, including the upper premaxillary margin in front of the nareal opening, the tip of the left nasal, part of the outer wall of the left canine alveolus, most of the third and fourth left premolars with parts of the adjacent palatal surface, some of the upper margin of both zygomatic arches and the left lambdoidal ridge. Fortunately it has been possible to reconstruct these from the opposite side in every case, and most of the restoration is indicated in outline in the drawings which, with the exception of Figs. 1, 3 and 8, have been traced from photographs, without attempt at correcting the deformation due to crushing.

Lateral Aspect Fig. 2.—One of the most striking features of the skull is the elongation of the face, anterior to the orbits, for the accommodation of the great sabres, and the noticeable lack of constriction back of, and of bulging opposite to, the alveoli of these teeth, best seen in the palatal view, Fig. 5. The orbits are probably more lateral in aspect than they would ordinarily be, owing to the flattening which the skull has undergone, and their lower margins lack the convexity which is so noticeable in *Hoplophoneus*, the surface instead sloping

upward and inward toward the eye socket as in *Smilodon*. The infraorbital foramen opens above the anterior half of pm. 3 and seems proportionately smaller than in a *Hoplophoneus* of

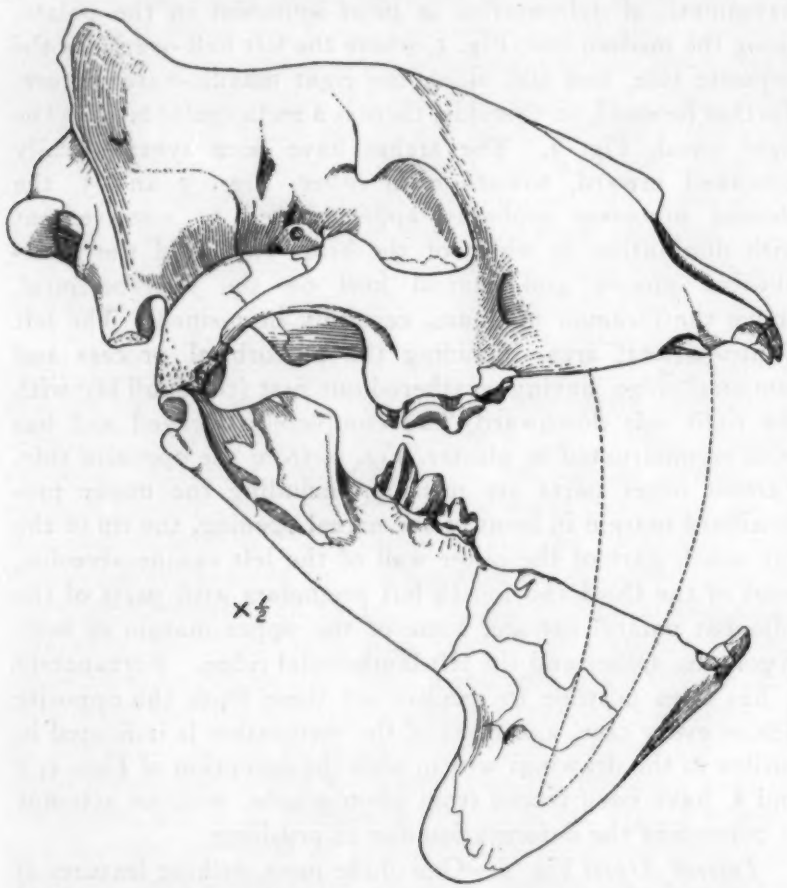

FIG. 2.—*Eusmilus sicarius* sp. nov. Skull and lower jaw from the right side, one half the natural size, drawn from a photograph and the specimen. Tip of jaw flange and parts of horizontal ramus restored, as indicated in outline. Width of upper canine correct, but length approximated.

comparable size. There are prominent postorbital processes, both frontal and jugal, the latter very wide, but barely discernible temporal ridges. Unusually deep pits appear within the temporal fossa for the origin of the temporal

muscles, more prominent perhaps in view of the feeble development of temporal ridges. The zygomatic arches are much flattened, concaved toward each other by crushing, Figs. 3 and 5, short and strongly arched upward, with immense elongated glenoid processes, the spring of the jugal arch, its lateral flattening and the elongation of the glenoids resembling the situation in *Smilodon* and perhaps correlatable with a similar dental reduction, huge canines, and immense gap of the jaws. Lambdoidal crests are prominent, as in *Hoplophoneus*, as is also the sagittal crest in the rear, but farther forward it dips downward as figured by the junior author in *Hoplophoneus oharrai*.[1] Its continuation forward to a point of confluence with the temporal ridges unfortunately cannot be traced as this part of the skull had completely decayed. The large foramen which, in *Hoplophoneus*, perforates the parietal near its occipital margin, just above the squamosal suture, here lies considerably farther up and, on the right side, has several smaller foramina in its vicinity, Fig. 2. The extreme prominence of the occiput and the forward slope of its central keel above the foramen magnum are probably accentuated to some extent by crushing which has, perhaps, also operated to rotate the condyles upward, so that they present more surface backward and outward than in *Hoplophoneus* and fail to project below the basioccipital plane. If this is their normal position it would increase the possibility of upward and backward tilting of the head and so promote the effectiveness of the downward plunge of the stabbing canines.

Within the temporal fossa, anterior to the sphenoidal fissure, there are three foramina, two large and circular opening laterally, one opposite the middle of the fissure (optic) and the other farther forward opposite its upper end. Still farther forward, and above the last mentioned foramen, is another, and smaller, one opening downward and forward. These are shown in Fig. 2. In their usual position are the large sphenopalatine, and below them the posterior palatine, canals. Below and back of the sphenoidal fissure is another opening, presumably the foramen rotundum.

[1] "The Black Hills Engineer," Vol. XIV., No. 2, 1926, p. 3.

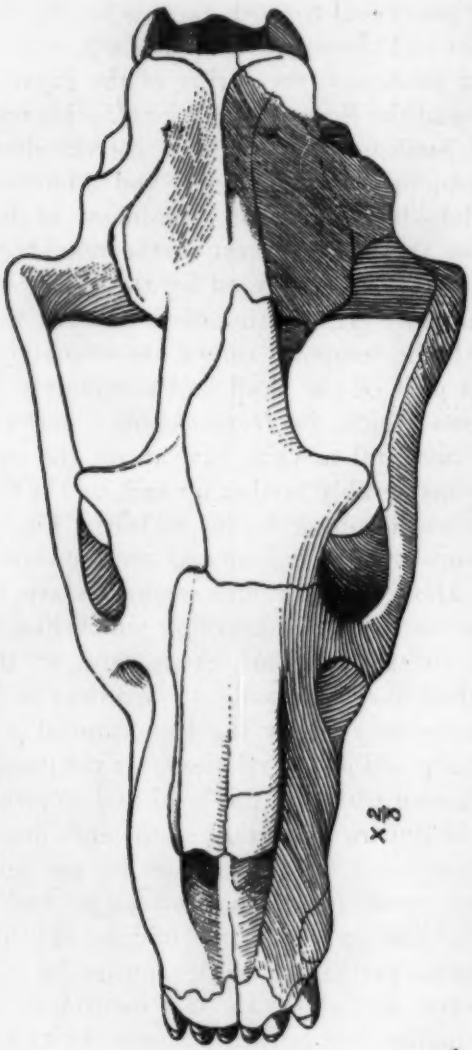

FIG. 3.—*Eusmilus sicarius* sp. nov. Top view of the skull, two thirds the natural size. Premaxillary margin, left lambdoidal crest and fronto-parietal area, tip of left nasal and margin of left zygomatic arch restored, as indicated in outline. Drawn from the specimen.

Dorsal Aspect, Fig. 3.—Seen from above, the skull is long and narrow, undoubtedly having had its transverse diameter decreased considerably by crushing. The long straight muzzle in front of the orbits is most striking as is also the small size of the brain case and the extreme degree of post-orbital constriction which separates it from the facial portion of the skull. Sutures are difficult to follow, that between the nasals being barely discernible and the fronto-nasal suture locatable with difficulty, due to the age of the animal. The part of the right frontal which remains does not reach the median line and the surviving portion of the temporal ridge is very faint. The symmetrical inward bending of the cheek arches seen in Figs. 3 and 5 is also a distortional effect. Owing to the backward projection of the occiput much of the supraoccipital surface and the condyles are visible when the skull is viewed from above.

Occipital Aspect, Fig. 4.—The excessive development of glenoid and mastoid processes can best be appreciated by viewing the skull from behind, Fig. 4. The glenoid processes are very long and wide, projecting proportionately farther below the tips of the mastoids than in *Smilodon*. Their posterior inner corners, at the glenoid margin, are not as greatly elongated as in other American sabre-tooths. Their entire lower surface is occupied by the glenoid fossae which are wider and shallower than in a *Hoplophoneus* of

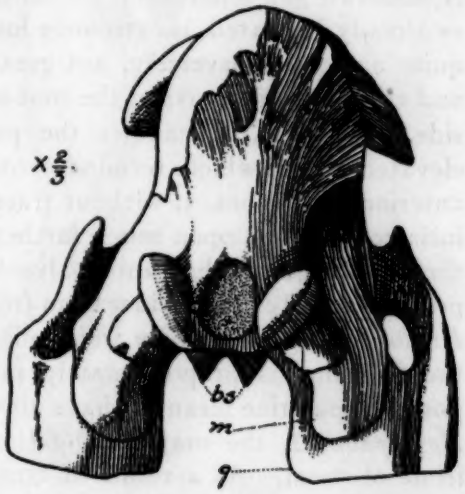

FIG. 4.—*Eusmilus sicarius* sp. nov. Back of the skull two thirds the natural size, drawn from a photograph and the specimen. Left lambdoidal crest restored from opposite side. *Bs*, Basisphenoid; *m*, mastoid process; *g*, glenoid process.

comparable size. The mastoid processes are proportionately larger than in *Hoplophoneus* with broad rugose tips for the origin of the cleido-mastoid muscles. There are, also, extensive areas on the postero-lateral surfaces of the processes for the origin of the sterno-mastoids. The function of these muscles, as explained by Matthew,[1] is to pull the head forward and downward on the neck, enabling a sabre-tooth to strike a tremendously powerful quick downward blow with the great canine teeth. The depth of the central groove in the basisphenoid, which seems to have been increased by the lateral flattening of the skull, may, also, be best appreciated by a reference to Fig. 4.

Palatal Aspect, Fig. 5.—Length and narrowness are again the two outstanding features of the skull when viewed from below. It is possible that the premaxillary border may have been more strongly arched forward than is shown in the figure, Fig. 5. It is intact up to the base of the left i. $\underline{2}$ and lacks very little of being complete elsewhere on its palatal surface. A moderate gap separates i. $\underline{3}$ from the canine alveolus, which, as already indicated, is extremely long from front to back and quite narrow transversely, not greatly flattened by crushing and still retaining parts of the root of the canine on the right side. Back of the canines the palatal margin has sharp elevated edges which terminate on either side against the anterior root of pm. $\underline{3}$, without trace of vestigial teeth. The incisive foramina open much farther forward with respect to the rear edge of the canine alveolus (*i.e.*, the canines are proportionately much wider from front to back) than in either *Hoplophoneus* or *Smilodon* and the length of the palate back of the foramina is proportionately much greater. The small posterior palatine foramina have about the same position as in *Hoplophoneus*, the major elongation of the palate lying in front of them. As a result of crushing the left half of the palate overlaps the opposite side, especially noticeable toward the posterior nareal border, where a large, presumably neurovascular foramen perforates the thickened palatal border on

[1] *Bull. Am. Mus. Nat. Hist.*, Vol. XXVIII., Article XXVI., 1910, p. 296.

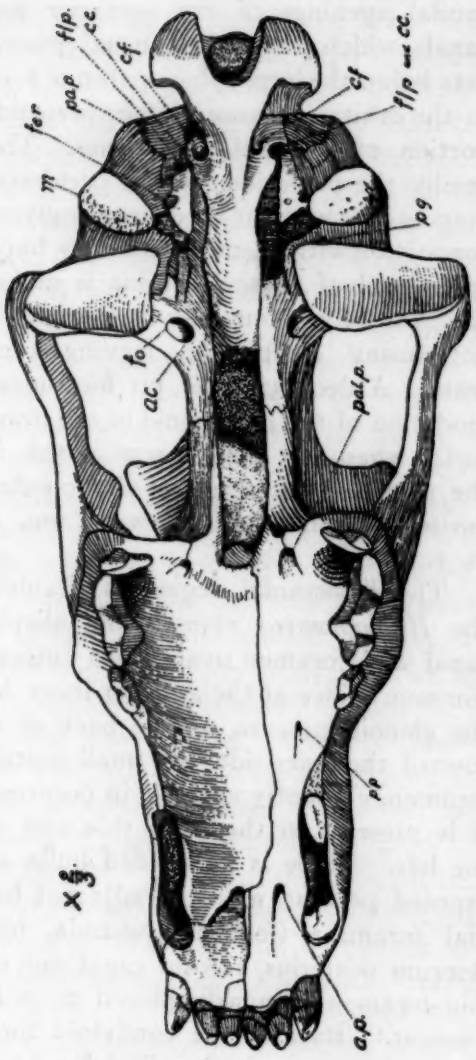

FIG. 5.—*Eusmilus sicarius* sp. nov. Palatal view of the skull, two thirds the natural size, drawn from a photograph and the specimen. *Ap*, Anterior palatine foramen; *pp*, posterior palatine foramen; *palp*, posterior palatine perforation; *ac*, alisphenoid canal; *fo*, foramen ovale; *pg*, postglenoid foramen; *m*, mastoid process; *fen*, fenestra rotunda; *pap*, paroccipital process; *flp*, foramen lacerum posterius; *cc*, carotid canal; *cf*, condyloid foramen. Left pm. $\frac{3}{}$ and $\frac{4}{}$ restored from the opposite side. Some plaster reconstruction on the left side of the palate not indicated in the figure. Part of outer side of left canine alveolus also restored.

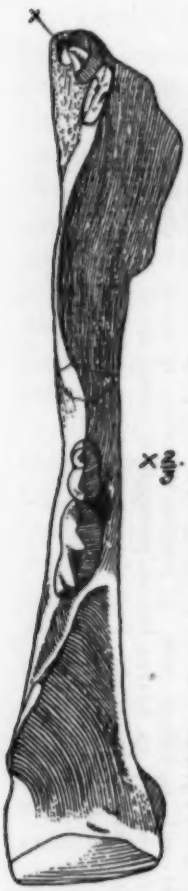

FIG. 6.—*Eusmilus sicarius*, sp. nov. Right half of the lower jaw, crown view, two thirds the natural size, drawn from a photograph and the specimen. Alveolus of median incisor indicated at *x*. Restoration of horizontal ramus in front of pm. $\frac{4}{}$ indicated in outline.

either side of the posterior nares (palp., Fig. 5). These are not to be confused with the caudal openings of the posterior palatine canals which have their usual position in cats below the large sphenopalatine foramina in the orbital expanse of the perpendicular portion of the palatine bones. They resemble the posterior palatine perforations in marsupials, without of course implying any connection with them. The bony bar which spans each of these foramina is not entire, the two halves touching in the middle but not fusing completely, leaving a central seam. A deep circular pit for the accommodation of the protoconid of the lower sectorial when the mouth was closed, lies at the posterior outer corner of the palate opposite the angulation between pm. $\underline{4}$ and m. $\underline{1}$.

The basicranial region is decidedly of the *Hoplophoneus* type. The alisphenoid canal and foramen ovale open into a large common orifice at the anterior inner base of the glenoid process, on the back of which, toward the inner side, is a small postglenoid foramen, evidently variable in occurrence as it is present on the right side and not on the left. There is no ossified bulla and an exposed petrous, with the adjacent basicranial foramina (fenestra rotunda, foramen lacerum posterius, carotid canal and condyloid foramen) similarly placed as in *Hoplophoneus*.[1] Back of the condyloid foramina the condylar margins are ill defined and the articular surface continues forward on the basioccipital.

[1] Compare Fig. 5 with Matthew, *loc. cit.*, p. 299, Fig. 6*B*.

Lower Jaw

Hatcher speaks of the downward projection of the flange as the most striking feature of the lower jaw of his type of *E. dakotensis*, but in *E. sicarius*, Figs. 2, 7 and 8, the flange is even deeper, while the jaw as a whole is a little shorter and much less robust. The flange is broadly concave externally and the sharp ridge which separates its lateral and anterior surfaces is convex forward as in the European *E. bidentatus* instead of straight as in *E. dakotensis*. Its tip is, unfortunately, incomplete and has been restored in plaster as indicated in the figures. A similarly incomplete fragment of the opposite flange, articulating accurately along the symphysis as shown in Fig. 8, suggests that the lower margin might have been a trifle deeper than in the reconstruction. The great depth and *narrowness* of the chin with its many minute, apparently vascular perforations in contrast with the very deep, *broad* and less perforated chin in *E. dakotensis* may be appreciated by comparing this drawing with Hatcher's Fig. 4 in the paper already referred to, where it will also be observed that the large foramen below the incisive alveolar border lies much closer to the latter in *sicarius* than in *dakotensis*. Judging from the position of the groove leading up to it, the mental foramen opened almost on a line with the lower margin of the ramus, but some of the fragments from which this part of the jaw was pieced together are missing and the orifice of the foramen is partly lacking. As shown in Fig. 2 it has been restored in plaster and is probably correct. Another fracture, with incomplete contact of the adjacent surfaces, traverses the jaw in the region of the small foramen just in front of pm. $\overline{4}$ mentioned by Hatcher, and we cannot be certain whether it was present or not in *E. sicarius*. As in *E. dakotensis* the outer surface of the ramus below the molars is convex longitudinally except for a depression at the alveolar margin below the point of contact of pm. $\overline{4}$ and m. $\overline{1}$. Some scorings on the elevated areas on either side of this depression look like rodent nibbling, but may be normal. The masseteric fossa is deep, unenclosed posteriorly and extends farther forward than

in *E. dakotensis*, terminating beneath the middle of the sectorial. The angular process is much weaker than in the last named species, has its point of origin much farther back toward the condyle and terminates in a small round end pointing outward and backward. The condyle is low, as in *E. dakotensis*, lying beneath the line of the alveolar border and similarly decreases in strength from within outward but is supported on a longer neck, while the coronoid process is much smaller and lower, not rising above the tips of the sectorial cusps. In the rear it slopes toward the condyle instead of having a vertical border as in *E. dakotensis*.

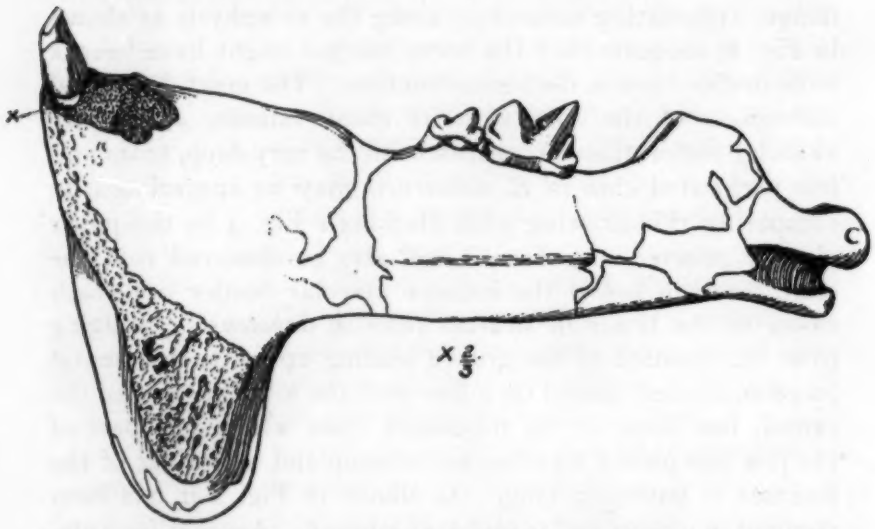

FIG. 7.—*Eusmilus sicarius* sp. nov. Right half of the lower jaw from the inner side, two thirds natural size, drawn from a photograph and the specimen. Alveolus of median incisor shown at *x*. Tip of chin flange and parts of horizontal ramus restored, as indicated in outline. Upper part of symphysis broken, and missing piece not recovered.

On the inner side of the jaw, Fig. 7, which is quite flat except for a shallow concavity parallel to its lower margin, the dental foramen may have been situated farther back than in *E. dakotensis*. Some unfortunately placed fracturing, with loss or decay of the essential fragments, makes its location uncertain and it has been omitted in the figure. The extreme

depth of the symphysis on the flange, and especially its median constriction with expansion above and below, seems to be of generic significance as all three of the known species of *Eusmilus* agree in these respects. Its lower half, however, occupies the center of the chin flange in contrast with both *E. dakotensis* and *E. bidentatus* where the symphysis lies along its anterior margin.

Between the canine and pm. $\overline{4}$ the alveolar margin is thin and sharp, with no trace of teeth. It bends downward abruptly in front of the premolar but rises rapidly beyond and continues forward as a sharp ridge which curves inward and then outward toward the canine, Fig. 6. The junction of this elevated margin with the concavity in front of pm. $\overline{4}$ is, unfortunately, interrupted by a transverse fracture along which the alveolar margin is missing and the contact is imperfect, so that the amount of downcurve of the anterior lower ramal margin in front of the fracture, with respect to the horizontal portion behind it, may be a little more or a little less than is indicated in the drawings, Figs. 2 and 7.

FIG. 8. — *Eusmilus sicarius* sp. nov. Front of right half of the lower jaw, two thirds natural size, drawn from the specimen, showing also a fragment of the left chin flange. Plaster restoration indicated in outline.

No other sabre-tooth could equal *Eusmilus sicarius* in ability to open its jaws which, in the absence of muscles, can now be swung backward a full 180° from their position of complete closure before the condyle begins to rise out of its socket. This is possible because of the great length of the glenoid process and the relatively slight elevation of its posterior inner corner, the shallow glenoid fossa, the pedunculate condyle and the small angular process which does not interfere with the rear of the glenoid process until the full 180° maximum of gap is attained.

RELATIONSHIPS

Whatever may be the affinities of *Eusmilus whitfordi* Barbour [1] it may be dismissed at once from consideration here

[1] *Nebraska Geol. Survey Publications*, Vol. 4, Part 17, 1915, pp. 235–238, Pl. I.

for it is certainly not *Eusmilus*, differing in the shape and proportions of the mandibular ramus and chin flange, in the shape of the symphysis and in the number of cheek teeth (two lower premolars) and agreeing only in the number of lower incisors.

So far as comparison is possible with the two previously known forms, *E. sicarius* occupies an intermediate position with respect to the shape of the lower sectorial from which metaconid and heel have vanished in *E. dakotensis*, but are represented by a single prominence in *bidentatus*, shown in Filhol's figures and emphasized in his text, where he describes this tooth as trilobate. If the skull of this old world form was known, differences might appear which would necessitate placing in separate genera the European and American forms now referred to *Eusmilus*.

As already indicated, both cuspidation and shape of the lower sectorials in *E. sicarius* suggest *Hoplophoneus*, with which the similar basicranial structure would also seem to indicate some degree of relationship, perhaps a common ancestor for both, despite the many differences referred to in the preceding pages. On previous occasions both the junior [1] and senior [2] authors have suggested the possibility of deriving *Eusmilus dakotensis* from certain deep-flanged species of *Hoplophoneus* from the Titanotherium beds. These speculations seem less probable now that the skull of an earlier and somewhat less specialized *Eusmilus* is known from a level well down in the Oreodon beds, and in view of the rather extreme degree of specialization already attained by the new form.

Perhaps we can most readily understand the dental and cranial peculiarities of *Eusmilus* if we visualize them as the result of extreme specialization in method of securing food, away from a biting habit and in the direction of maximum stabbing efficiency. It is rather curious, however, if this idea is correct, that the same end should have been accomplished by forms as far apart as *Eusmilus* and *Smilodon*, each provided with tremendous sabres, the former attaining the maximum

[1] *The Black Hills Engineer*, Vol. XIV., No. 2, 1926.
[2] *Proc. Am. Phil. Soc.*, Vol. LX., No. 2, 1921.

of jaw flange development known in any Machaerodont, while the latter has practically lost it. Whatever we think of Matthew's discussion of the phylogeny of the Felidae, his conclusion[1] regarding *Eusmilus* that "this genus is more progressive than *Hoplophoneus*, but apparently out of the direct line of descent" both of true cats and Machaerodonts, seems fully justified by the new material.

MEASUREMENTS

Skull.	Mm.
Prosthion to basion	187
Prosthion to nasion (approx.)	102
Basion to nasion (approx.)	117
Prosthion to bregma	?
Basion to bregma	?
Bizygomatic diameter at glenoid processes	91
Diameter postorbital constriction	21.5
Postorbital constriction to tip of postorbital process	36.5
Postorbital constriction to apex of occipital crest	57.5
Bimastoid diameter, maximum	61
Palatal length (prosthion to pterygoid processes of maxillæ)	125
Posterior palatal breadth at m. $\underline{1}$ (greatly decreased by crushing)	34
Infraorbital foramen to posterior maxillary margin	40.5
Tooth row, c to m. $\underline{1}$ inclusive	87.5
Tooth row, pm. $\underline{3}$ to m. $\underline{1}$ inclusive	36
Diastema, c-pm. $\underline{3}$	21
Diastema, i. $\underline{3}$-c	7
Canine alveolus	32 × 4.5 (at middle)
Pm. $\underline{3}$ maximum length of crown	9
Pm. $\underline{3}$ maximum width of crown	4
Pm. $\underline{4}$ maximum length of crown	22.5
Pm. $\underline{4}$ maximum width of crown	8
Lower jaw.	
Mandibular ramus, length	167
Depth of symphysis	84
Depth posterior to pm. $\overline{4}$	33
Depth of flange, as restored, from rear edge of canine alveolus	91
Length pm. $\overline{4}$-m. $\overline{1}$, maximum, at alveolar border	31
Diastema c-pm. $\overline{4}$	58.5
Pm. $\overline{4}$, maximum length of crown	11.5
Pm. $\overline{4}$, maximum width of crown	6
M. $\overline{1}$ maximum length of crown	19.5
M. $\overline{1}$ maximum width of crown	8.5
Distance from coronoid process to bottom of jaw	38

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[1] *Loc. cit.*, p. 314.

DEVELOPMENT OF AMERICAN PALÆONTOLOGY

By WILLIAM BERRYMAN SCOTT

(*Read April 30, 1927*)

In a section of one of the many scientific congresses which were held at St. Louis in connection with the World's Fair of 1904, the question concerning the position of palæontology among the sciences was debated by the late Dr. John M. Clarke, State Geologist of New York, and Prof. Henry Fairfield Osborn, President of the American Museum of Natural History. As was almost inevitable, each of these distinguished men of science regarded the question from the standpoint of his own specialty. Clarke, having the invertebrates in mind, contended that palæontology should be considered as a department of geology; while Osborn, thinking only of the vertebrates, maintained that palæontology should be classed with zoölogy. The debaters were thus talking about quite different things; for, illogical as it may seem, to separate radically two parts of the same science, in practice the separation cannot well be avoided. The palæontology of plants and invertebrate animals is so interwoven with geology as to be an indispensable part of that science; while the interest and significance of fossil vertebrates are chiefly zoölogical, their geological value being distinctly subordinate. Especially has this been true since the publication of Darwin's *Origin of Species* in 1859, because the study of extinct vertebrates has supplied some of the most striking and cogent evidence that has been brought forward in support of the theory of evolution. Indeed, as Darwin tells us in his autobiographical sketch, the theory was first suggested to his mind by the wonderful fossil mammals which were found buried in the Pampas of Argentina.

In the brief sketch which I am about to lay before you, I shall confine myself to Vertebrate Palæontology, not only because of my much greater familiarity with that branch of the subject, but also because of the limitations of space and time. It is peculiarly fitting that an account of the origin and development of vertebrate palæontology in America should find a place in the programme of the bicentennial celebration of the American Philosophical Society, because the greater part of this immense body of labor has been accomplished by the members of this Society, and its publications have, from the beginning, been the medium through which many notable discoveries have first been announced. In saying this, I am not unmindful of our illustrious members, such as Conrad, Say, Gabb, Clarke, and Walcott, who devoted their lives to the study of fossil invertebrates with such wonderful results. I am merely pointing out that the field of vertebrate palæontology in this country is more peculiarly the preserve of the American Philosophical Society, and emphasizing the appropriateness of making especial mention of it on this occasion.

Until nearly the end of the eighteenth century, the Aristotelian conception of the *scala naturæ* and the indestructibility of species was held, as a matter of course, by almost all naturalists and philosophers and, so long as such views prevailed, there could be no science of palæontology. Fossils, it is true, had attracted the attention and interest of intelligent men from the remotest times, and are mentioned by writers from Herodotus to Leonardo da Vinci; but in those allusions or discussions, it is taken for granted that the fossils, if they were organic at all, belonged to the same kinds of animals and plants as those now living on the earth. The palæontology of the vertebrates may be said to date from 1796, when Cuvier demonstrated that the fossil elephants of France and Italy did not belong to any existing species. Before that, the occurrence of unmistakable elephant bones in Italy and southern France had been explained by assuming that these were the remains of elephants which Hannibal had brought with him in his great march from Spain. Cuvier's demonstration would

not seem to have attracted very much attention, but the discovery which he announced in 1799 went around the world like an earthquake shock, and was received with hardly less consternation. In this memoir, the great naturalist described a number of mammals, of which the partial skeletons had been found in the gypsum quarries of Montmartre; and he showed beyond peradventure that these mammals belonged to extinct species, genera, and even families. The discovery, thus announced, began a new epoch in human thought, sweeping away definitively the doctrine of the indestructibility of species.

Before the scientific period began in this country, there was a long time of more than a century, in Colonial and early Republican days, when fossil bones, especially those of the American Mastodon, were the sport of the amateur, and astonishing work he made of it. Many years ago, I undertook the task of preparing a bibliography of the American Mastodon, which led me through a maze of old and forgotten magazines, newspapers, and the proceedings of learned societies, and resulted in the accumulation of an extraordinary mass of most amazing absurdities. A survey of this nonsensical stuff I published in *Scribner's Magazine* for 1887 under the title "American Elephant Myths," and it may not be out of place to give a few samples of it here. Both in Europe and America the bones of large fossil creatures were generally supposed to have belonged to gigantic men. The great fossil salamander of Oeningen, in Switzerland, was named by Scheuchzer, of Zurich, *Homo diluvii testis*, the man who had been a witness of the deluge. The shoulder-blade of an elephant was formerly exhibited in a Venetian church as that of St. Christopher, and in the cathedral of Lucerne was another. Father Kircher, the German Jesuit, whose ponderous work, *Mundus subterraneus*, etc., was published in 1678, showed the absurdity of believing that giants of 60 feet or more in height could ever have existed and, as the only alternative, explained the huge bones as sports of nature (*lusus naturæ*) generated in the soil by the *vis plastica* of the

earth. Notwithstanding this lucid demonstration, belief in the giants persisted till a much later date and on both sides of the ocean.

Cotton Mather, in a letter published in the *Philosophical Transactions* of the Royal Society for 1714, confirms the Scriptural account of antediluvian giants, just as St. Augustine had done, by describing elephant bones and teeth. "There was another tooth, near a pound heavier, found near the banks of Hudson's River, a great way below the surface." Governor Dudley, of Massachusetts, wrote of these same remains to Cotton Mather, that he was "perfectly of the opinion that the tooth will agree only to a human body for whom the Flood only could prepare a funeral." Don Quixote was of the same opinion, for he says: "In the Islands of Sicily there have been found long bones and shoulder bones so huge that their size manifests their owners to have been giants, as big as great towers. This truth geometry states beyond doubt."

When president of this Society, Thomas Jefferson presented a paper, read on March 10, 1797, upon certain bones found in the floor of a cavern in Greenbriar County, Virgi ia. These bones he compared with those of a lion, and concluded that the dreadful creature, which he named "the Great Claw, or Megalonyx," was more than three times as large as the lion of to-day. "A difficult question now presents itself. What is become of the great-claw?" "In the present interior of our continent there is surely space enough . . . for mammoths and megalonyxes who may subsist there. Our entire ignorance of the immense country to the West and North-West, and of its contents, does not authorise us to say what it does not contain." Believing, as he did, in the indestructibility of species, Mr. Jefferson brought forward all the hunters' tales which he could collect and which pointed to the existence of huge and terrible beasts of prey in the Appalachian forests. "In fine, the bones exist: therefore the animal has existed. The movements of nature are in a never ending circle. The animal species which has once been put into a train of motion, is still probably moving in that train. For if one link in

nature's chain might be lost, another and another might be lost, till this whole system of things should evanish by piecemeal." "P. S. March 10, 1797. After the preceding communication was ready to be delivered in to the Society, in a periodical [1] publication from London I met with an account and drawing of the skeleton of an animal dug up near the river La Plata in Paraguay." This skeleton was named *Megatherium* by Cuvier, who recognized its affinities to the armadillos, sloths, and other South American Edentata. Evidently Mr. Jefferson's faith in his terrible leonine creature still inhabiting the western forests was shaken, and he thought that *Megalonyx* and *Megatherium* might turn out to be the same, when more complete material of the former had been recovered. "It may be better, in the mean time, to keep up the difference of name." (*Trans. Amer. Philos. Soc.*, Vol. IV., p. 246 ff.).

Even after he had been elected to the presidency of the United States, Mr. Jefferson kept up his lively interest in geology and palæontology; and he sent Captain William Clark, the famous explorer, on a collecting expedition to the Big Bone Lick in Kentucky. It was at his instigation that Dr. Samuel L. Mitchill, one of the most distinguished of the early American naturalists and a Senator from New York, translated Cuvier's "Theory of the Earth."

The American Mastodon was a fascinating problem to the pseudopalæontologists of Europe and this country, most of whom identified him with the Mammoth. Buffon has some exquisitely absurd remarks on this animal. According to Jefferson (*loc. cit.*, p. 267) Buffon "assumes the tusks of the mammoth to have been those of an elephant, some of his teeth to have belonged to the hippopotamus, and his largest grinders to an animal much greater than either." In the same Vol. IV. of our *Transactions* is a paper by George Turner, read on July 21, 1797, "on the Extraneous fossils, denominated Mammoth Bones, etc.," in which he says: "Deposits of his remains are very frequently found in Siberia

[1] *Monthly Magazine*, Sept., 1796.

and other parts of the old world. In North America they are abundant. The countries bordering upon the Ohio and its tributary streams, have already furnished numerous discoveries of the kind." "The late Dr. W. Hunter was the first to relieve the learned world from an error they had long indulged. Having carefully compared a few specimens of the American bones with others of the Siberian nondescript, and these again with similar parts of the elephants, hippopotamus, etc., he became convinced that the two first were vestiges of one and the same species of animal; but differing essentially in size and form from the bones of any other at present known to us: that, consequently, they were not parts of the elephant, nor of the hippopotamus, but of some huge carnivorous animal."[1] "Had the opportunities of this accurate observer been greater than it appears they were; or, in other words, had his materials been less scanty, he would have discerned the remains of a second incognitum, whose stature was not, perhaps, inferior to that of the other. These second remains evince a member of the *herbivorous* order; and, from their extraordinary size, I have no hesitation in believing that they belonged to some link in the chain of animal creation, which, like that of the Mammoth, has long been lost."

Turner, it appears, had accepted Cuvier's ideas of extinct species, and apologizes to Mr. Jefferson for being reluctantly constrained to differ with him. He winds up his dissertation with the following eloquent passage: "With the agility and ferocity of the tiger; with a body of unequalled magnitude and strength, it is possible the Mammoth may have been at once the terror of the forest and of man! And may not the human race have made the extirpation of this terrific disturber a common cause?" The article to which Turner refers was published in 1767 by William Hunter, who says: "If this animal was indeed a carnivore, which I believe cannot be doubted, though we may as philosophers regret it, as men we cannot but thank heaven that his whole generation is probably extinct." This is one of the earliest suggestions that species may become extinct.

[1] *Trans. Roy. Soc.*, Vol. LVIII., p. 42.

Even after palæontology had emerged as a true science, pretentious ignorance long continued its amusing antics. The skeleton of the American Mastodon, for example, which has long been mounted in the British Museum, was taken to London in 1839 by its discoverer, and there exhibited as a specimen of the "Leviathan of Holy Writ," and was visited by curious crowds; it is mentioned in the diary of Fanny Burney, the younger. So late as 1840, a mastodon, mounted like a biped and with the missing cranium restored in rawhide, was exhibited through the southwestern states as the skeleton of a giant. The proprietor had a trunk full of physicians' certificates attesting that these were human bones.

After this, perhaps, unduly long account of the amateurish beginnings of palæontology in America, we may turn to the origin and development of the real science. That development may be appropriately divided into four periods, each distinguished by outstanding characteristics. Of these periods, the first may be taken as extending from 1797 to about 1847, and during that time vertebrate palæontology was almost entirely in the hands of men who had had a medical education as, in those days in English-speaking countries, the medical school offered the only means of obtaining the indispensable knowledge of anatomy. Dr. Caspar Wistar, Professor of Anatomy in the University of Pennsylvania, and Dr. Harlan were the most successful cultivators of the science in Philadelphia, Dr. De Kay and Dr. Mitchill in New York, and Dr. John Warren in Boston. Furthermore, in this early period, the material consisted of the bones found in deposits of very late geological date, such as limestone caverns, superficial swamps and bogs. Nothing was known, or even suspected, of the wonderful treasures which were found at a later time in the Far West, in the Great Plains and Rocky Mountain regions. So far as I have been able to discover, the first paper by a competent American writer on a palæontological subject was by Dr. Wistar, for many years the president of this Society, and bears the title: "A Description of the Bones deposited, by the President, in the Museum of this Society."

The paper bears no date of presentation, but is included in Vol. IV. of the *Transactions*, which is dated 1797, and was probably read in December of that year. In this description Dr. Wistar does not criticize, or even refer to, the opinions of Mr. Jefferson, but merely points out the unmistakable Edentate affinities of *Megalonyx*. Nor does he deal with the problem of extinction. For many years after this date, there were very few publications dealing with vertebrate palæontology, because of the scarcity of the material. Many remains of *Mastodon* were found, especially in the Hudson valley, and the existence of horses in Pleistocene America was established, but not much else was learned.

The second period in the development of our science may be said to have begun about 1847, and to have continued for some thirty years, terminating somewhat vaguely, for the third period was well under way before the second ended. This second period was dominated by the great figure of Joseph Leidy, who long possessed nearly a monopoly of vertebrate palæontology in this country. Not only is the period characterized by its great and almost solitary worker, but also by the progressive discoveries of the marvelous succession of Mesozoic and Tertiary animals which were entombed in the soft rocks of the high plains. Fur traders from St. Louis were the pioneers in these discoveries, and Dr. Prout, of that city, published in 1846 the description of some very large, but imperfect, grinding teeth, which he mistakenly, though not unnaturally, attributed to Cuvier's genus *Palæotherium*. This was one of the mammals found in the gypsum of Montmartre and described in Cuvier's epoch-making paper of 1799.

From 1847 to his death in 1891, Leidy published a nearly continuous series of papers and monographs upon the Tertiary mammals and the Cretaceous reptiles of our own Western country, with some additional material from the green sands, or so-called marls, of New Jersey. Leidy himself did no collecting, and never even visited the fossil beds of the Western bad lands until 1877, when he made an excursion to

Fort Bridger in southwestern Wyoming. He was thus dependent for material upon what others sent him and, for a long time, any interesting fossil vertebrates, found anywhere in the United States, were sent to Leidy as a matter of course. Especially important was the collecting done for him by Dr. F. V. Hayden, long the head of the U. S. Geological and Geographical Survey of the Territories. Hayden was so fortunate as to be considered mad by the Indians, who looked upon him as inviolable and sacrosanct; consequently, he was able to visit fossil grounds inaccessible to any other white man.

In addition to the host of shorter papers, Leidy published four great quarto volumes upon this Western material. The first, issued in 1853 by the Smithsonian Institution and remarkable for its beautiful lithographic plates by Sonrel, was devoted to the fossils, mostly mammals, of the White River beds of what was then Nebraska Territory. In 1865 he put out the "Cretaceous Reptiles of the United States," and this was followed, in 1869, by a still larger work upon the White River fossils, with much more extensive and better preserved material; and that work remains, to this day, the solid and enduring foundation of all the work done on the White River fauna. From the opening of South Dakota and northwestern Nebraska to safe exploration, to the present day, hardly a season has passed without some collecting in the White River country by parties from the universities and public museums of the country, and this assiduous work has resulted in the gathering of an enormous mass of material, of an unrivalled completeness of preservation. In no other formation within the limits of North America is there such a wealth of beautiful fossil mammals. It is a very remarkable fact that so little has been added to the list of genera and species discovered and named by Leidy, when the hap-hazard way in which his material was gathered is duly borne in mind. On the other hand, the even more important work of supplementing Leidy's descriptions has been carried on with conspicuous success. Forms which he knew only from a jaw fragment, or a few teeth, or, under the most favorable conditions, from a good

skull, are now represented in many collections by a series of wonderfully perfect skeletons. From the biological point of view, our knowledge of the White River fauna has proved to be of exceptional importance, because of its clear and cogent evidence for the theory of evolution, and the cognate problems of geographical distribution and the migrations of the various mammalian orders.

Next in importance, perhaps, is the work Dr. Leidy did upon the Eocene mammals of the Bridger basin in southwestern Wyoming. Here, again, he was a pioneer, opening the field to his successors and, by his preliminary studies, rendering their task incomparably less difficult. These Eocene fossils were described in a succession of brief papers, the results of which were brought together and extended in his "Contributions to the Extinct Vertebrate Fauna of the Western Territories," one of the quartos of the final reports of the Hayden Survey. Pleistocene mammals received considerable attention, and Leidy put out a series of monographs dealing with them, one on the Extinct American Ox-tribe, one on the Peccaries, and others on the Gigantic Ground Sloths or Gravigrada. This work, however, was merely an extension of the palæontological researches of Wistar, Harlan, De Kay and others, such as had been published from time to time since the beginning of the century.

As though all this mass of work were not enough, Leidy added much to our knowledge of fossil reptiles, and was the first to give an account of a fairly complete dinosaur skeleton, *Hadrosaurus*, which he obtained from the Cretaceous greensand of New Jersey. His dinosaur material from the Western Cretaceous was extremely fragmentary, but he made good use of it; and he made many valuable contributions upon the better preserved tortoises, crocodiles, and lizards of the Cretaceous and Tertiary.

The reader of Leidy's works on fossil vertebrates cannot but be struck by the strictly objective nature of his discussions, for he has hardly anything to say of the theoretical inferences which might be drawn from the great body of

material which was at his disposal. This was not due to any lack of interest, or want of constructive imagination; for, in a paper published in 1847, he plainly, though not explicitly and in set terms, expresses his belief in the theory of evolution. That was twelve years before the appearance of Darwin's "Origin of Species," and in 1872 he extended this conception to the origin of Man. In view of this daring and extremely progressive attitude on Leidy's part, it seems very remarkable that he has so very little to say on topics of this nature in his palæontological work. I cannot with any confidence explain this reticence of his, because, well as I knew him and often as I discussed various problems with him, this particular one never came up in our conversations. It is my belief, however, that he felt that any deductions or inferences that he might draw would be necessarily premature; that the first task of the American palæontologist should be to assemble his material, arrange it in taxonomic order, and make it as complete as possible, and then proceed to see what theoretical interpretation it would best bear. Though this assumption of mine is mere conjecture, it is so in harmony with Leidy's general attitude as to be probable.

The third period in American vertebrate palæontology began about 1869, long before the end of Leidy's life, and continued for the remainder of the nineteenth century. This period is pre-eminently characterized by the work of two very able and distinguished men, Professor E. D. Cope, of Philadelphia, and Professor O. C. Marsh, of Yale. Not only did these great investigators throw themselves into the work with a zeal and energy that was beyond praise, but they had great sums of money at their disposal, an advantage which Leidy had never had, and feeling unable to bear the financial competition involved, he gradually gave up palæontological research in favor of other lines of inquiry.

It is impossible, within the limits of such a summary as this, to give any adequate conception of the series of brilliant discoveries made by these men, discoveries which astonished and delighted the whole scientific world. When I was a

student in Huxley's laboratory in 1878–9, I can well remember the excitement and intensity of interest which these discoveries aroused in London. It is only to be regretted that the bitter and deadly quarrel between these two really great palæontologists should, in some degree, have prevented their services to palæontology from being even greater than they were. This was one of the famous feuds, of which there have been so many in the history of the natural sciences, as of Cuvier and Lamarck in France, Owen and Huxley in England, Weismann and Eimer in Germany, Moreno and Ameghino in Argentina. Happily, this is a fashion which seems to have passed away from the contemporary world. Aside from its indescribably great intrinsic value, it is particularly fitting and proper that the work of Cope should be commemorated on this occasion, for so many of his discoveries were first announced in this room, and the publications of this Society contain so large a proportion of his papers.

Both Marsh and Cope were thoroughgoing evolutionists, and Darwin's great conception was the mainspring of their work, which, far from being solely descriptive, was very largely theoretical and deductive. They both covered nearly the entire range of North American fossil vertebrates, Fishes, Amphibia, Reptiles, Birds and Mammals. For the first time in the history of vertebrate palæontology in America, skilled collecting was developed. Both Cope and Marsh took the field in person, and they gradually trained a corps of workers, who have never been surpassed in skill and competence. Most of these men were of the Far West, and had long been interested in the mysterious petrified bones so abundant in the bad lands. Some had had the advantage of an education; others, so far as geology and palæontology were concerned, had educated themselves, following a strong native bent toward the natural sciences. Conspicuous in this group were Sam Smith, of Fort Bridger, and L. S. Davis, of eastern Oregon; they published nothing, but as collectors, their services to science were great and lasting.

The late Mr. J. B. Hatcher had a genius for finding and

extracting the most delicate fossils, though he was far more than a collector; his lamentably early death cut short a brilliant career as a palæontologist, upon which he had hardly more than embarked. He began his career as a coal miner in Iowa; and the beautifully preserved fossil plants of that field so aroused his interest, that he went to Yale University in order to study under Professor Dana. After graduation, Hatcher became Marsh's collector-in-chief in the organized field work which Marsh was doing, both for the Yale Museum and the U. S. Geological Survey. He devised methods by means of which it is practicable to bring in the most fragile and delicate of specimens, keep them safe through their long, rough journey over rudimentary roads and in jolting freight trains. Hatcher obtained an incredible quantity of the most beautiful material, not only for Marsh, but also for Princeton University and the Carnegie Museum in Pittsburgh.

Similar skillful and competent work was done by the late Dr. J. L. Wortman, first for Cope, subsequently for the American Museum of Natural History. Both Wortman and Hatcher trained a corps of able assistants, and all modern collecting is done according to their high standards, even by men who were never with them directly. New methods and devices pass rapidly from hand to hand. More than anything else, I think, it was this astonishing improvement in the art of collecting and preparing fossils which made the period of Marsh and Cope a new epoch in the history of American palæontology. Able as these men were, they were not more so than Leidy; but they had incomparably better material to work with, and all their successors have enjoyed the advantages of this revolution in the methods of collecting.

In attempting to summarize the work accomplished by Marsh and Cope, I am conscious of the impossibility of doing justice to these eminent men within such narrow limits of space and time; their work was as individual, and as strongly contrasted, as were the temperaments of the two men. Marsh had an extraordinary flair for the striking, one might almost say the sensational, and therefore his discoveries

aroused a widespread popular interest, which was not given to Cope's no less important and original writings. The finding in the chalk of Kansas of birds with teeth and with other reptilian characteristics was one of Marsh's achievements which was "heard round the world," especially when his great and beautifully illustrated quarto volume, describing the almost complete skeletons of these wondrous birds, was published. Hardly any single discovery in palæontology was more strikingly confirmatory of the theory of evolution than the unearthing of toothed birds.

Marsh's only other large monograph was that on the remarkable group of Eocene hoofed animals which he named the Dinocerata, and which have been facetiously described as "six-horned elephants, only they weren't elephants and hadn't any horns." They were nearly elephantine in size and appearance, and had three pairs of horn-like protuberances on the skull. This volume was likewise most elaborately illustrated with beautiful lithographic plates. Indeed, Marsh regarded his plates as much the most valuable part of his work. He used to say that descriptions and theories became obsolete and were superseded, but that "good figures were a permanent contribution to any department of Natural History."

Of even greater popular interest was Marsh's announcement that he had been able to decipher the genealogy of the Horse, the development of which had taken place chiefly in North America. When Huxley was in this country in 1876, he gave a series of lectures in New York on Evolution, and one of these, entitled "The Demonstration of Evolution," was devoted chiefly to Marsh's discoveries, and particularly the development of the horses.

The fossils collected, for the most part by Hatcher, enabled Marsh to publish a really wonderful series of discoveries among the dinosaurs, those huge and bizarre reptiles which once dominated all the continents; some of these, especially the quadrupedal horned dinosaurs, were among the strangest creatures that ever lived. Marsh's discoveries made dinosaurs

familiar to every one and even the newspapers nowadays make sport of them, though the reporters labor under the strange delusion that all fossil animals lived at the same time and put dinosaurs and mammoths into the same picture.

Still another capital discovery of Marsh's was the finding of rich faunas of mammals in the Jurassic and Cretaceous periods; Jurassic mammals had been known in Europe, especially in England, but Marsh added much to our knowledge of these early and primitive types. This is, by no means, a catalogue of Marsh's remarkable discoveries, but it includes the principal ones. In 1880 Sir Archibald Geikie published in *Nature*, under the title "Yale College and Palæontology," a very laudatory account of Marsh's contributions to our science.

Cope's work lends itself even less than that of Marsh to summary statement, nor did it arouse any such general popular interest; while his lack of conciliatory diplomacy and his combative disposition made him a host of enemies. Nevertheless, Cope's name is one of the most illustrious in American science, and in sheer force of intellect he has rarely been surpassed. When I was a student in London, Professor Seelye, a distinguished English palæontologist, once remarked to me: "I regard Cope as the only young man of genius in either hemisphere;" rather a sweeping statement, perhaps, but significant none the less. In mammals, Cope determined several genealogical series, notably among the rhinoceroses and the camels; he discovered and determined the faunas of the Paleocene, previously entirely unknown in this country, and he did a wonderful piece of work in his determination of the lowest Eocene, or Wasatch, fauna, nearly all our knowledge of which is due to him. Many mammalian orders, such as the Condylarthra, Amblypoda, Tæniodonta, Creodonta, were first recognized and named by Cope. The Deep River, or Middle Miocene, and the Blanco, or Middle Pliocene, were his discoveries; and no observer contributed so much to the elaboration of our knowledge of the divisions, subdivisions, and correlation of the continental Tertiary formation of the

West as did Cope. Leidy and Marsh contented themselves with very vague and general correspondences between the American and European Tertiary; but Cope, who had studied in France and was familiar with the geology of that country, made much more exact and minute correlations, and these are, to a very large degree, still in use. Such correlations are of the highest value in all discussions of the origin of mammalian groups and their subsequent migrations.

Among the reptiles, Cope made many discoveries; he was the first to bring to light the extraordinary Permian reptiles of Texas, and he did much to elucidate the mosasaurs, or sea-dragons, of the Cretaceous. He made a number of interesting and important discoveries on the dinosaurs, but his dinosaurian material did not compare in extent and perfection with that which Marsh had at his disposal.

A department of palæontology which Cope had almost to himself was that of the fishes. It is true that Professor Newberry had done fine work in Palæozoic and Triassic fishes, and Leidy and Marsh had described some excellent material of the fishes of the Green River Eocene; but in the Cretaceous and Tertiary fishes of North America, no one could even distantly rival the work of Cope.

Cope's published work is of enormous volume; by far the greater part of it consists of short papers, but in the final reports of the Hayden Survey two of the quarto volumes are by Cope, one on Cretaceous reptiles, the other a huge and portentous tome on Tertiary vertebrates, the second part of which was never published, though the plates had already been lithographed and printed. Many years after Cope's death, Dr. W. D. Matthew issued these plates, together with those of Cope's suppressed volume on Permian reptiles. The reasons for the non-publication of all this mass of invaluable work, which was ready for the printer, were not creditable to American science or to the United States Government of the day, but it would serve no good purpose to disinter these ancient scandals here.

Both Cope and Marsh died before the nineteenth century

had quite come to an end; but just as they had begun their investigations while Leidy was still the commanding figure in the vertebrate palæontology of North America, so the younger generation began to come to the fore while Marsh and Cope were still in their prime. During the twentieth century the work has greatly expanded, many more investigators and more institutions having taken it up. The principal disadvantage against which this science has to contend is the great cost which it involves. The field expenses of collecting parties are very heavy, as the men must travel long distances, and must be paid and maintained while at work. Much more costly is the work of preparing the material for study and exhibition in the museum, for this can be done only by men of great skill and experience and must be done very slowly and deliberately. A mounted dinosaur skeleton usually represents the expenditure of many thousands of dollars. Naturally, it is the public museums, with large revenues, which can most efficiently perform these very expensive tasks.

The fourth and last period in the history of American vertebrate palæontology may be said to begin with the last few years of the twentieth century, and to continue to the present time. Nearly all of the prominent figures in this period are those of living men, and it is an invidious and thankless task to compare their work. However, it would be absurd to omit all mention of the great collections, unrivalled in the world, which have been brought together in our museums. The American Museum of Natural History in New York is the leader in this work, and its collections of vertebrate fossils are by far the finest ever made. Professor Osborn and his associates, Messrs. Matthew, Gregory, Granger, Brown and others, have the high credit of this great achievement. The National Museum in Washington has extremely fine material, much of it dating from Marsh's day and greatly increased by Gilmour and Gidley. The Carnegie Museum of Pittsburgh, long under the direction of Dr. W. J. Holland, has great collections, gathered by Hatcher, Peterson, and Douglass. In Chicago, Dr. E. L. Riggs has done notable

work in collecting for the Field Museum of Natural History. The Natural History Museum of Denver, Colorado, is becoming exceedingly rich in the fossil mammals of that state, thanks chiefly to the labors of Mr. Harold Cook. A position quite unique in the science is held by the County Museum in Los Angeles, where the late Mr. Darrow exploited the tar-pits of Rancho la Brea, with astonishing results. The museum is devoted almost exclusively to the Pleistocene, and in all the world there is no such collection, either in extent, variety, or in perfection of preservation, of the Pleistocene mammals.

Several of the universities have done admirable work in these lines; at Yale, Professor Lull and his associates continue the studies inaugurated by Marsh, and at Amherst Professor Loomis has made notable collections in both North and South America. At Princeton, Professors W. J. Sinclair and M. S. Farr have carried on, with brilliant success, the tradition inaugurated half a century ago. Marsh gathered around him at New Haven a remarkably able corps of assistants, who highly distinguished themselves at subsequent times. George Baur, in Chicago, inaugurated the University museum, and was eventually joined there by S. W. Williston, one of the foremost authorities on fossil reptiles, who had made the fine collections at the University of Kansas. Another of Marsh's associates, Professor E. H. Barbour, has created the admirable museum of the University of Nebraska. One of the greatest of American university collections is that at Berkeley, California, made illustrious by the researches of Dr. J. C. Merriam, now President of the Carnegie Institution of Washington.

For the sake of completeness, I should perhaps add the two great Canadian collections, one at the University of Toronto, under Professor Parks, the other the Victoria Museum at Ottawa. The late Mr. Lawrence Lamb, and the veteran collector, C. S. Sternberg, who had found many of the gems in the collections of Cope and Marsh, inaugurated this work, which is being carried on by the latter's son, Mr. C. M. Sternberg. I had the pleasure of visiting the Ottawa Museum a short time ago, and was quite unprepared for the magnificent

assemblage of beautifully preserved dinosaurs which is there displayed; a series second only to that of the American Museum.

While not attempting to estimate the value of the investigations carried on by men who are still actively at work, we may yet point out some of the salient features of this fourth period of American vertebrate palæontology, in which we are still living. The first and most obvious characteristic of the period is the immense extension and diffusion of interest in the science among all classes of people. For a long time Philadelphia and Washington had the only important collections, while now the well-equipped museums extend from coast to coast and number at least sixteen. Such a film as Conan Doyle's *Lost World* would have been impossible only a few years ago, not only from insufficiency of knowledge, but also from want of popular interest and understanding. This great increase in the number of museums has necessitated a corresponding increase in the number of workers and in the amount of the material gathered, until this material has gained colossal proportions.

A second characteristic, which cannot but impress the onlooker, is the manner in which American collectors, finding their own continent too small, have reached out into other fields. The first of these foreign expeditions of which I have heard are the three sent out by Princeton University to Patagonia, 1896–1899. These were planned and executed, with extraordinary success, by the late Mr. J. B. Hatcher and Mr. O. A. Peterson, now of the Carnegie Museum. The third and last of these trips was made in coöperation with the American Museum of Natural History. The stately series of quarto volumes, fifteen in all, dealing with these collections will, I trust, be completed before the end of the present year. This publication was made possible by the generosity of the late Mr. J. Pierpont Morgan; and the plan for that series of volumes, like everything else about these expeditions, was due to Mr. Hatcher. Two other series of Patagonian explorations and collecting parties were those from Amherst College in 1912,

under Prof. F. B. Loomis, and those from the Field Museum of Natural History of Chicago, under Dr. E. S. Riggs. The success of Professor Loomis' work is known to all palæontologists, and I hear privately that Dr. Riggs is achieving magnificent results.

The other foreign expeditions are those which have been sent out by the American Museum, and which are additional to the many famous collecting trips in zoölogy and anthropology, conducted by that institution. American Museum parties have made collections of great value in Patagonia, in Egypt, in the Siwaliks of India, and in the Gobi Desert of Mongolia. The latter expeditions, under the leadership of Dr. Roy Chapman Andrews and with Mr. Walter Granger as palæontologist, have been of the very first order of importance and have become famous throughout the world. Not only have the collections made thrown a flood of light upon many problems of importance only to men of science, but "finds" of what may fairly be called sensational interest have delighted intelligent laymen in all lands. I refer, of course, to the discovery of dinosaur nests and eggs, the nature of which has been clearly demonstrated by the embryonic skeletons within the eggs.

In short, the main characteristic of this modern period of American vertebrate palæontology is its extraordinary development, both extensive and intensive. The beginning of this work was in the papers by President Jefferson and Doctor Wistar, read in this room nearly 130 years ago. Little did those eminent men imagine what was to be the outcome of their modest endeavors.

In the coming June, it will be fifty years since Osborn and I, immediately following our graduation, went West as members of the first of the many palæontological expeditions sent out from Princeton. It is not for me to attempt any estimate of the far-reaching consequences which that holiday journey was to have. I wish, in closing, to dwell for a moment on another side of that undertaking, namely the romance and joy of it all. At that time, and for a good many years later, collecting

expeditions in the Far West had to be self-contained and self-supporting. Traveling in the field was all done on horseback, and for many months the collector's home was his tent. A four-horse wagon to carry supplies and to transport the packed and boxed fossils for long distances to the nearest railroad, was a necessity, as were also the teamster and cook, one functionary or two, according to the size of the party. The country was open in every direction, unsettled and unfenced, save for an occasional cattle-ranch at long intervals. In most of the regions where fossils were to be found, the summer climate was delightful, with little or no rain and always cool nights, while the days, whatever the thermometer might say, were made very tolerable by the extreme dryness of the air and the consequent evaporation from the skin. Until the end of the century there were several regions where hostile Indians gave a spice of danger to the work, which did not detract from the pleasure of it all. Quite aside from the scientific importance of the work, the free and adventurous life, the strange wild country and its strange people, the magnificent scenery and the sense of novelty, all made the life fascinating to the dweller in cities. Of the many students who were with me on one or another of my Western trips, I do not know of one, however perfunctory his palæontological interest, who does not look back on that experience as one of the most memorable and delightful of his life. That day is gone, never to return. Fossils are there in abundance still, as they always will be, but the adventure and the romance have departed, flying from the wire fence and the Ford car.

P.S. These sentimental reminiscences have been awakened by reading Dr. W. D. Matthew's "Early Days of Fossil Hunting in the High Plains." He says:

> "The old delightful camping routine, with its pioneer equipment and unhindered freedom of exploration has shifted and changed to automobile travel. . . . The high plains, too, will always be a great fossil field, but the romance of their discovery and exploration by the early fossil hunters belongs to the past."

AGE CHANGES IN ANTHROPOLOGICAL CHARACTERS IN CHILDHOOD AND ADULT LIFE

By CLARK WISSLER

(*Read April 30, 1927*)

One striking characteristic of our national development has been the emphasis placed upon the education of our children. Far more than any other country our fathers taxed themselves to provide free schooling for every child, and to-day the most imposing building to be seen in an American village is the public school. Nor have we stopped with mere book learning, but have added to the school staff the nurse, the doctor, and the dentist. Naturally, this trend in our culture has quickened an interest in the growing child, so that to-day we can boast a science of growth, not merely based upon stature and weight, but upon other body and face measurements, observations upon dentition and radiographs of the bones. But it is upon the child in school that this interest is focussed. Now, however, the thoughtful among us have awakened to the contrast in our treatment of the child in school and the young adult out of school. When the youth leaves school, where everything possible was done for him, he steps out into life, where he must shift for himself. (Of course, we refer to those unable to enter college.) Whether this is as it should be, is not to be considered now, but there is a movement well under way to provide adequate adult education and, as may be anticipated, educators, sociologists, psychologists, etc., are turning to the adult. They are investigating such problems as: What are the age changes in ability to learn? Do adults require more time to master a subject? etc.

Since it was the educational interest in the growing child

that stimulated the study of growth during the school period, we may anticipate increased attention to the analogous age changes in the bodies of adults.

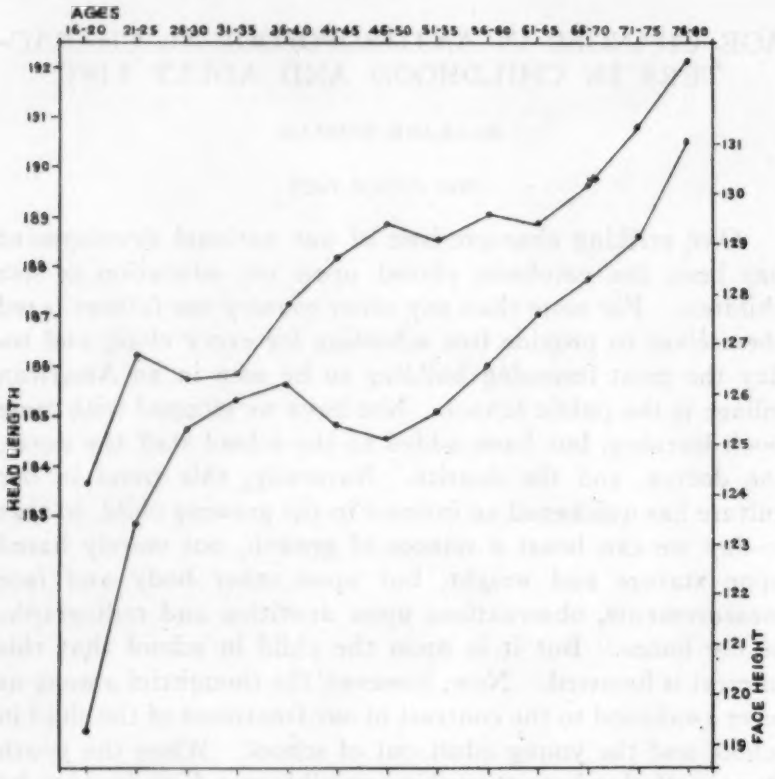

FIG. 1.—Growth curve for length of head and face height.

Those of you familiar with the literature of anthropometry and anatomy, know the usual assumption to be that growth of the body ends shortly after the high school period. The stature and the diameters of the head and face are believed to be so near their ultimate size at entrance to college, as not to change thereafter. Likewise, form is assumed to be fixed at an early age, especially the cephalic index, the relation between the length and breadth of the head, in which no significant change has been admitted after the elementary school

period. In any case, the assumption is, that by the age of twenty-five, or thereabout, the skeleton has become stable, both as to size and form, and consequently, that when seeking size and form norms for a population, individuals of twenty-five will be equivalent to those of sixty.

Yet, notwithstanding these traditions, contemporary literature is replete with suggestions that changes in size and form are characteristic of adult life and there are not wanting hints that the skeleton is itself sensitive to age and nutritive conditions. During the World War, when famine prevailed in parts of Russia, Ivanovsky made numerous repeated observations upon famished adults; his results show losses in stature ranging from 3.8 to 6.6 cms., for males; 3.6 to 4.8 cms., for females. In case of the head, there was a shrinkage in size, the width of head shrinking more than the length, resulting in a falling index, or a change in form. In similar fashion, the breadth of face decreased to a greater degree than the length, thus changing the proportions of the face; finally, the nose changed markedly in size and proportion. Many other changes are noted by this observer, but we may pass over them with the general statement that changes in size and form of the adult body can occur in times of famine.

In contrast to these observations and at about the same time, Doctor Hrdlička, in Washington, was collecting data on Old Americans, presumably all well fed. Each individual was measured but once, but when grouped according to age, certain differences were noticed: as a slight increase in head diameters during adult life, the face tending to grow wider, the nose to become longer, etc.

Naturally, the measurements so far cited were upon the living, and one may question as to whether the bones play any part in these changes. Famine may have merely wasted the soft parts of the body, and likewise advancing age may cause shrinkage of tissue. Fortunately, some observations have been made upon the skeleton itself. No doubt many of you are familiar with the work of T. Wingate Todd, of Cleveland, who possesses a very large series of human skeletons for

which the ages at death are known. From the study of such aged skeletons, Todd has devised a technique for estimating the age of any skeleton whatsoever, to an accuracy of less than two years. By this method, any considerable collection of skeletons from a single population group can be used for the study of age changes. Further, Doctor Milo Hellman of New York has made observations upon the bones of the face, showing progressive age changes in size and proportion throughout adult life.

Yet, all the observations so far recorded are in the nature of pioneer efforts and the intense exploration of adult age-changes is still for the future; it is moreover an innovation, running counter to deepseated assumptions, and so invites further pioneer exploration, which is the justification for this paper.

There is on file in the American Museum of Natural History a series of anthropometric measurements for Hawaiians, made by the late Doctor Louis R. Sullivan under the auspices of the Bishop Museum in Honolulu. For the ages from six to twenty there are records for 316 boys and 323 girls; for adults, 337 males and 253 females. The ages of the adults, as recorded, range up to eighty years. This series gives a race population sample, covering the whole life-span, except the first years of childhood. All the individuals included in this series were recorded as of pure descent. Further, all the observations were taken during the course of a single year.

Doctor Sullivan died before working up this material, so a study of the data has been made by the writer. The averages and variabilities were worked out in detail; taking the children in groups of yearly ages, the adults in periods of five years each. Thus we can plot curves of progressive age change from six to eighty years, and for males as against females. This is not the time to inflict upon you the details of this study; they will be published in due time; but the general outcome of the investigation can be stated. To simplify matters we shall use the data for males only.

In stature and width of head there is a progressive increase to the age of 36 to 40; after this age, both stature and width of head shrink steadily to extreme old age.

Length of the head increases, the longest heads in our series occurring after sixty years of age.

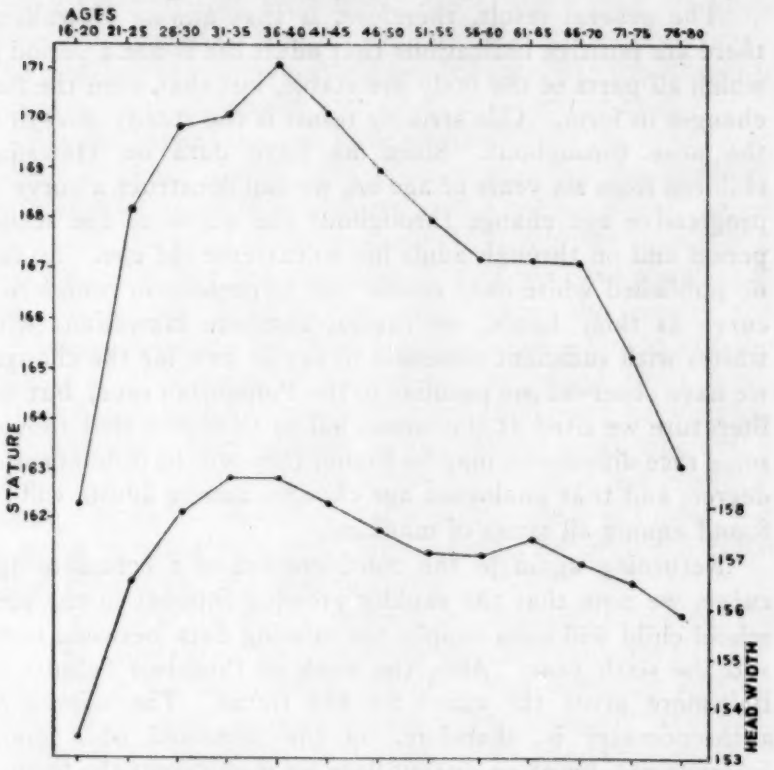

FIG. 2.—Growth curve for stature and width of head.

The height of face, in popular speech, continues to increase throughout, or the face grows longer with age. The width of face, on the other hand, changes but slightly, if at all.

The nose grows continually wider and longer.

The proportions of the head and face show changes consistent with the above noted age-shifts in absolute diameters.

So far we have used the data for males only, and need but

add that, in the main, the females show parallel age curves to those of the males. There are some sex differences in the amount of age change and the periods for maximum change. In general, then, the data for one sex confirm the findings for the other.

The general result, therefore, is that among Hawaiians, there are positive indications that adult life is not a period in which all parts of the body are stable, but that even the face changes in form. One striking result is the steady growth of the nose throughout. Since we have data on Hawaiian children from six years of age on, we can construct a curve of progressive age change throughout the whole of the school period and on through adult life to extreme old age. So far, no published white data enable one to present so complete a curve as this; hence, we cannot compare Hawaiians with whites with sufficient closeness to say in how far the changes we have observed are peculiar to the Polynesian race; but the literature we cited at the outset led us to expect that though some race differences may be found, they will be differences of degree, and that analogous age changes among adults will be found among all types of mankind.

Returning again to the consideration of a complete age curve, we note that the rapidly growing interest in the pre-school child will soon supply the missing data between birth and the sixth year. Also, the work of Professor Schultz in Baltimore gives the curve for the fœtus. The science of anthropometry is, therefore, on the threshold of a great achievement, for at no distant date we may expect the formulation of continuous age curves, from the first weeks of fœtal life to old age. For white races, it is even now possible to give, in a tentative way, such a curve for the form of the head. Thus, the head of the fœtus is round, but grows steadily longer and narrower until birth; then during the first year or two, the head again grows rounder, or wider; then when the child enters school, his head is again growing longer and relatively less wide, which process continues at diminishing rate through adult life.

Thus we see that in the matter of growing a head, the individual starts with a round head, ending in old age with a longer head. In other words, after the first year of life, the cephalic index is falling. And, we have seen that this process is characteristic of the fœtus. So in general, except for a temporary reaction just after birth, one can say that the head grows from round to long, from conception to the adult level. This is to say that a very round-headed race, like the Koreans, begins in early childhood with an extreme round head.

Besides Hawaiians, we have in hand data for other race groups in those islands, and when comparing the heads of children with those of adults, we note a curious thing: as we have said, the head of the young child is rounder than it will ever be again, but the unpublished data we have show, that the rounder the head in the adult stage, the greater the differences between the child and the adult. Or, to put the matter another way, to achieve a very round adult head, nature begins with an extremely round child head, backing up, as it were, to a running start for the old age goal; whereas, if an adult long head is the objective, the start is less vigorous. What may be the meaning of this peculiarity is not clear, but it seems to point to a close inter-relation between the racial form of head and the growth curve.

As stated at the outset, space is not available to present the detailed tabulations for the series of Hawaiians used in this study; but we believe that the preceding summary and the citations of such meager evidence as is readily available, reinforces the suggestion that age-changes in adult population groups should be seriously studied before we can consider the science of growth a well-rounded sector of human biology. So far, the tendency has been to focus all studies of growth upon our school population and all anthropological research upon the average adult level; but we now have before us indiçations that the whole life span is a series of progressive changes, suggesting that no part of the total curve of growth and age change can be fully understood when taken alone.

SMOOTHED AVERAGES FOR HAWAIIAN MALES

AGE	CEPHALIC INDEX	FRONTO-PARIETAL INDEX	CEPHALO-FACIAL INDEX	ZYGO-MATICO-FRONTAL INDEX	ZYGO-MATICO-GONIAL INDEX	FACIAL INDEX	NASAL INDEX	EAR INDEX
6–10	84.7	70.1	84.3	83.1	77.2	85.3	76.1	56.3
11–15	83.7	70.5	86.5	81.5	76.9	86.3	76.1	55.0
16–20	83.0	70.2	89.0	78.9	76.5	87.1	76.9	52.9
21–25	83.7	69.1	90.0	76.8	76.1	87.6	77.5	52.0
26–30	84.8	68.3	90.7	75.3	76.2	87.7	77.8	52.0
31–35	85.3	68.0	91.0	74.4	76.4	87.3	78.0	52.0
36–40	84.7	68.1	91.7	74.2	77.0	86.8	78.6	52.0
41–45	84.0	68.2	92.2	73.9	77.3	86.1	79.2	51.2
46–50	83.4	68.3	92.3	74.1	77.6	86.1	79.3	51.3
51–55	83.5	68.1	92.5	73.6	77.5	86.5	79.0	51.2
56–60	83.2	67.9	92.5	73.4	76.8	87.1	78.7	51.1
61–65	83.5	67.7	92.6	72.0	76.7	87.5	78.1	51.1
66–70	83.1	67.1	93.2	72.3	76.3	87.8	77.3	50.3
71–75	82.5	66.8	92.5	71.9	76.3	89.6	75.3	49.7
76–80	81.5	66.9	92.0	71.6	75.7	91.6	73.4	48.9

SMOOTHED AVERAGES FOR HAWAIIAN MALES—*Continued*

AGE	STATURE	HEAD LENGTH	HEAD WIDTH	MINIMUM FRONTAL	FACE WIDTH	FACE HEIGHT	NOSE HEIGHT	NOSE WIDTH
6–10	133.3	173.3	147.2	103.1	124.1	105.9	45.9	35.0
11–15	147.3	178.5	149.9	105.6	129.9	112.4	48.7	37.1
16–20	162.2	183.6	153.4	107.9	136.8	119.3	52.1	39.9
21–25	168.0	186.3	156.5	108.1	140.8	123.4	54.1	41.4
26–30	169.9	185.7	157.9	107.8	143.0	125.3	55.1	42.4
31–35	170.1	185.9	158.6	107.5	144.3	125.9	55.7	42.7
36–40	171.1	187.4	158.6	108.1	145.6	126.3	55.9	43.5
41–45	170.0	188.3	158.2	108.0	145.7	125.4	55.7	43.9
46–50	168.9	188.8	157.5	107.7	145.3	125.1	55.5	43.8
51–55	167.9	188.6	157.2	107.3	145.7	125.6	56.3	44.2
56–60	167.2	189.1	157.2	106.7	145.6	126.5	56.8	44.4
61–65	167.2	188.8	157.4	106.4	145.7	127.5	57.8	44.8
66–70	167.1	189.7	157.0	105.7	146.0	128.4	59.0	45.1
71–75	165.2	190.7	156.6	104.1	144.5	129.2	60.2	44.8
76–80	163.5	192.2	155.8	102.6	143.1	131.1	61.3	44.5

THE ORIGIN OF MAN FROM THE ANTHROPOID STEM—WHEN AND WHERE?

By WILLIAM K. GREGORY

(*Read April 30, 1927*)

Professor Osborn has recently submitted the evidence for the existence of man in the Pliocene age in Europe, Asia and even North America.[1] He has also argued that for certainly more than one million years past our ancestors have been erect-walking, large-brained, speaking men, not differing in essentials from the human genus of to-day and contrasting profoundly with the arboreal or semi-arboreal great apes.

This immense vista of man's antiquity and of his aloofness from other mammals has called forth more than one expression of thankfulness that the much maligned human race has at last been freed by anthropological science from a degrading sense of kinship with apes and monkeys—repulsive creatures whose very names in ancient and modern times have been used in contempt and derision. This bar sinister in man's reputed pedigree has been viewed with horror by many anti-evolutionists, who have sought by every artifice of rhetoric to discredit the idea; but scientists of world-wide reputation have also striven either to secure a verdict of "not proven" or to establish a complete alibi for mankind. Linnæus, accepting the Miltonic cosmogony, placed man only a grade lower than the angels and at the head of the legions of organized creatures; but in recognizing that man is a mammal, and more particularly, a member of the order of Primates, Linnæus opened the way for Darwin and Huxley. Sir Richard Owen, on the contrary, although admitting that man is structurally a mammal, set him apart in splendid isolation,

[1] At a meeting of The New York Academy of Medicine, April 7, 1927, and at the Bicentenary of the American Philosophical Society, April 29, 1927.

the sole representative of a subclass Archencephala. And now Professor Osborn hands down a decision at first sight quite adverse to the claims of the anthropoid tribe to the place of honor as man's next of kin. Specifically, he holds [1] that these animals "constitute a separate branch of the great division of primates, not only inferior to the Hominidæ but totally disconnected from the human family from its earliest infancy."

But, like the slave in the classical story whose unpleasant and doubtless risky duty it was to remind royalty "*Memento te hominem esse*," I conceive it as my hard duty to remind mankind that these poor relations of ours, mute witnesses of the past, are still with us and that the evidence of our lowly origin can hardly be waved aside on the ground of the length and aloofness of our own lineage.

If we concentrate our attention on the evidence for the, at first, almost inconceivable antiquity of man as an independent phylum, we may easily forget that the Pliocene epoch is next to the nearest to us of a long line of known geologic epochs, most of which are many times longer than the Pliocene itself. If we accept Barrell's estimates based on the rate of disintegration of uranium into lead and helium, we find that even the Lower Pliocene is only some six million years distant from us, while the beginning of the Eocene is set down as some sixty million years ago. And what is this in turn, compared with the 700 millions of years since the beginning of the Palæozoic? At most, the human race has then been proved to be a superior line of its own for less than one hundredth part of the time that bivalve molluscs have been separate from univalves or sponges from corals.

Man is not the only mammal of the Pliocene epoch that was already substantially like his modern representatives. Palæontologists have shown that the same is true of the horses, tapirs, rhinoceroses, elephants, bears, pigs, monkeys, apes and many other mammals. In other words, the amount of evolution that has apparently taken place since Pliocene times in all these groups appears at first sight to be surprisingly slight. Professor Osborn has been a leader in

[1] "Evolution and Religion in Education," 1926, p. 136.

dividing up the old families of horses, rhinoceroses, proboscideans, and other groups, and in tracing the lines backward through a long series of Tertiary horizons, along very gradually converging lines, very few of which are definitely known to run together even so far back as the Lower Eocene. However, if the geologic epochs themselves extend as many millions of years as they are supposed to do, is it any wonder that the old "phylogenetic trees" have been lengthened into clusters of nearly parallel lines, converging only at extremely distant points? But because horses and asses may have been on separate lines far longer than was formerly thought, are they in reality any less nearly related to each other than they were before? And in general, are zoölogical relationships, *e.g.* of the horse to the rhinoceros and the tapir, really altered because our ideas of the antiquity in years of all creatures has been greatly expanded?

Partly because the amount of evolution since Pliocene times appears at first sight to have been very slight and because evolution in the horses, proboscideans, and many other phyla has in most cases been extremely slow throughout the Tertiary period, it might be suspected that evolution in man has been equally slow and that man will be proved to be distinct from all other phyla as far back as the families of horses, rhinoceroses, proboscideans, etc., were separate from each other; that is, as far back as at least the Eocene epoch. Accordingly, Dr. Roy C. Andrews intends to search for the early forerunners of man in the Upper Eocene and Lower Oligocene formations of Mongolia.

However, Professor Osborn and other palæontologists have demonstrated that among the swarming phyletic lines of Tertiary perissodactyls accelerations and retardations of the general evolutional advance were frequent. Primitive horses, with undiminished side toes and short-crowned teeth, in general characteristic of the Oligocene epoch, persist in the Miocene, side by side with far more progressive phyla with reduced side toes and long-crowned teeth, leading to modern horses. Relatively primitive titanotheres (*Telmatherium*

ultimum), with barely perceptible horn swellings above the orbits, were contemporaneous in the Upper Eocene of Utah with much more advanced forms (*Eotitanotherium*, *Protitanotherium*) with well developed horns. So, too, it is generally recognized that certain groups have changed but little during enormous reaches of geologic time, while others have become profoundly specialized during the same period. Among the mammals, the opossum has come down to us with only slight modifications in the dentition from the primitive marsupials of the Upper Cretaceous. The family of horses, on the other hand, during the same period underwent intensive modifications. In view of all this, where is the direct evidence that the evolution of man has proceeded at approximately the same average rate as that of the horse and his congeners, and that the two families date back equally far in geologic time?

Before taking up the direct palæontological evidence on this matter, let us consider several lines of indirect evidence. If we examine the crown patterns of a large series of upper molar teeth of modern horses, asses and zebras, we find the specific distinctions between them are minute and difficult to determine, and we are struck with the comparative uniformity or relatively narrow range of variability in pattern. On the other hand, an equal number of normal human molars will reveal far higher variabilities in pattern both in the human species as a whole and in the various races. A typical second upper molar of an Eskimo, for example, has a secondarily simplified crown with only three main cusps, while the corresponding tooth of American Indians is often a vigorously developed quadrangular tooth with four main cusps, but within the confines of other races individuals of either extreme type may occur. Similarly, the lower molars vary from a five-cusped type with the "*Dryopithecus* pattern" of the grooves and furrows, to a four-cusped form with a cruciform pattern. Among the extinct Hominidæ the Piltdown lower molars preserved the *Dryopithecus* pattern intact, while the Heidelberg molars show the beginning of the cruciform

pattern.[1] Dr. Hrdlička and others have shown the wide variability in proportions of the fossil and recent human molars. Among the recent orangs, chimpanzees and gorillas the variability of each premolar and molar tooth in pattern and dimensions, as recorded by Remane, Hrdlička and others, is enormous. Such exceptional variability, which is thus a general characteristic both of men and of the anthropoid apes, seems indicative of hereditary instability and of relatively rapid evolutional divergence.

If man and ape had parted company as long ago as did tapir and horse, their relatively higher instability should have made their molar patterns far more different from each other than those of tapir and horse, whereas, as we shall see, the contrary is the fact.

Professor Osborn long since showed how the three main cusps and the fourth cusp or hypocone of human upper molars severally correspond with those of the Eocene primates. The four cusps of the upper molars of modern anthropoid apes have also been traced back to the same ground plan. Professor Osborn has also shown that the upper molar patterns of the horse and tapir, now excessively diverse in appearance, have likewise been derived from a primitive upper molar pattern with three main cusps and a low posterointernal cusp, all severally corresponding to those of the ancestral primates.

Now if we assume for a moment that chimpanzee and man began to diverge from each other and from the primitive Eocene primates as long ago as horse and tapir did from the primitive Eocene perissodactyls, then, if evolution is on the whole equally slow in each group, the amount of difference between the upper molar patterns of modern chimpanzee and modern man should be roughly equal to the amount of difference between the upper molar patterns of modern tapir and modern horse. But the facts do not support this as-

[1] Gregory, William K., 1915, "Studies on the Evolution of the Primates," Parts I., II., *Bull. Amer. Mus. Nat. Hist.*, XXXV., Art. XIX., pp. 239–355. Gregory, William K., and Hellman, Milo, 1926, "The Dentition of *Dryopithecus* and the Origin of Man," *Anthropological Papers Amer. Mus. Nat. Hist.*, Vol. XXVIII., Part I., pp. 1–123, Pls. I–XXV.

sumption. Fig. 1 shows at once that the difference between the first pair is far less than that between the second. Moreover, the difference in the upper molar patterns between the Pliocene ancestor of the chimpanzee (*Dryopithecus rhenanus*)

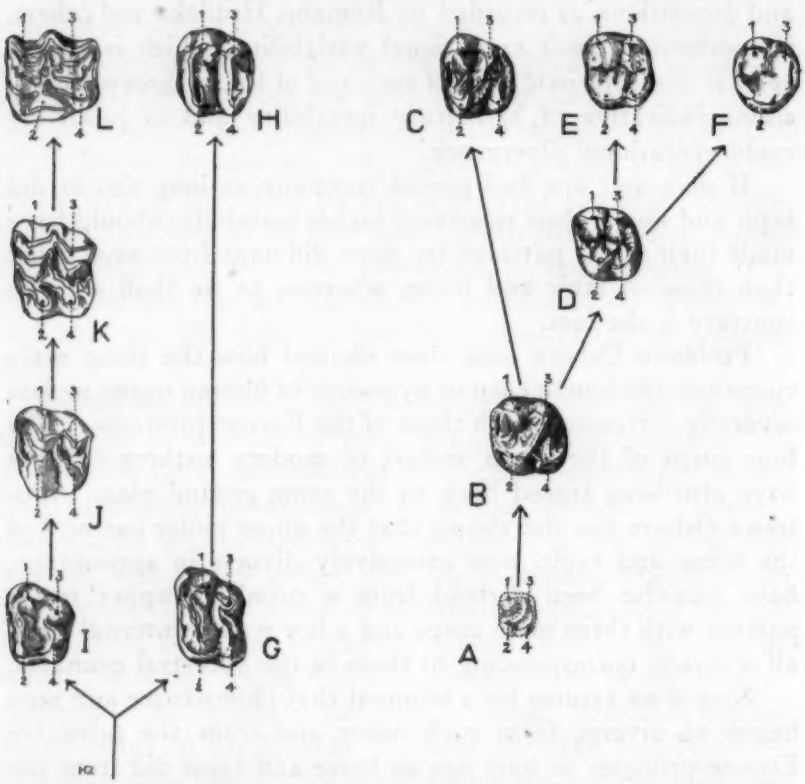

FIG. 1.—Comparative evolution of the upper molar patterns of horses, tapirs, anthropoids and man: *A* to *F*, structural series leading to the chimpanzee and to man; *G*, *H*, Eocene and modern members of the tapir family; *I* to *L*, *Eohippus* to modern horse; *A*, *Propliopithecus*, hypothetic reconstruction to fit known lower molars of this form; *B*, *Dryopithecus rhenanus*, Pliocene; *C*, recent chimpanzee; *D*, Neanderthal man (Le Moustier); *E*, modern Australian aborigine; *F*, modern Eskimo; *G*, Eocene ancestor of tapir (*Systemodon*); *H*, modern tapir; *I*, Eocene ancestor of horse (*Eohippus*); *J*, *Miohippus; K*, *Parahippus; L*, *Equus*. FIGS.—*J*, *K*, *L*, after Matthew.

and the oldest known human upper molars is evidently slight, while the difference between tapir and horse is profound.

Inspection of Fig. 2 will further show that in the lower

molars also the difference between modern chimpanzee and modern man is far less than that between modern tapir and modern horse, while the difference between the Pliocene

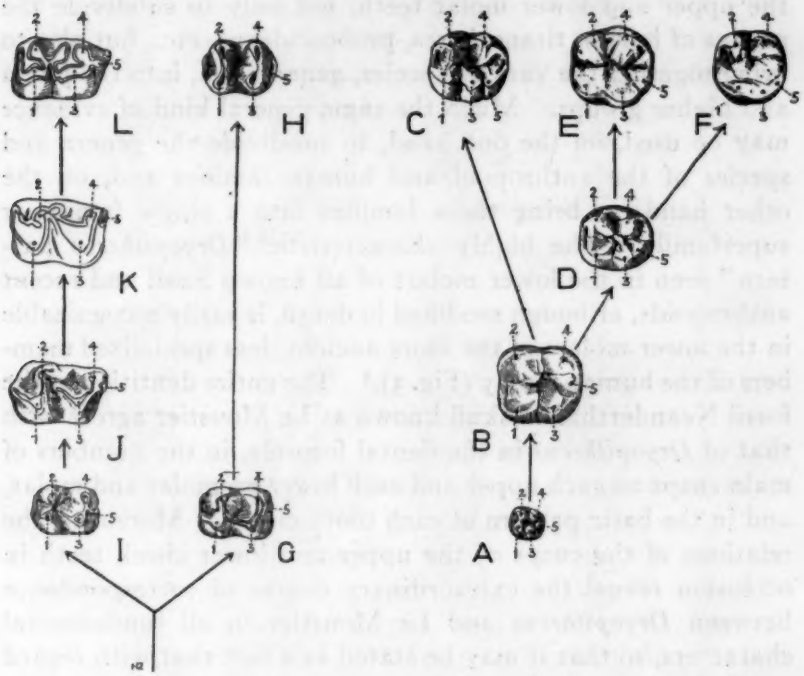

FIG. 2.—Comparative evolution of the lower molar patterns of horses, tapirs, anthropoids and man: *A* to *F*, structural series leading to the chimpanzee and to man; *G*, *H*, Eocene and modern members of the tapir family; *I* to *L*, *Eohippus* to modern horse; *A*, *Propliopithecus*, known lower molars of this form; *B*, *Dryopithecus rhenanus*, Pliocene; *C*, recent chimpanzee; *D*, Neanderthal man (Le Moustier); *E*, modern Australian aborigine; *F*, modern Eskimo; *G*, Eocene ancestor of tapir (*Systemodon*); *H*, Modern tapir; *I*, Eocene ancestor of horse (*Eohippus*); *J*, *Miohippus*; *K*, *Parahippus*; *L*, *Equus*. FIGS.—*J*, *K*, *L*, after Matthew.

ancestor of the chimpanzee and the Piltdown lower molar is almost negligible as compared with the difference in the lower molar patterns of tapir and horse. Hence, so far as these comparisons may be of value, they are quite unfavorable to the view that the structural gap between chimpanzee and man is as great as that between tapir and horse. On the contrary, they suggest that the divergence between chimpanzee and man from a common anthropoid stock may be far more recent

than the divergence of tapir and horse from a primitive perissodactyl stock.

Palæontologists constantly use the detailed patterns of the upper and lower molar teeth, not only to subdivide the groups of horses, titanotheres, proboscideans, etc., but also to bring together the various species, genera, etc., into the phyla and higher groups. Much the same general kind of evidence may be used, on the one hand, to subdivide the genera and species of the anthropoid and human families and, on the other hand, to bring these families into a single family or superfamily. The highly characteristic "*Dryopithecus* pattern" seen in the lower molars of all known fossil and recent anthropoids, although modified in detail, is easily recognizable in the lower molars of the more ancient, less specialized members of the human family (Fig. 3).[1] The entire dentition of the fossil Neanderthaloid skull known as Le Moustier agrees with that of *Dryopithecus* in the dental formula, in the numbers of main cusps on each upper and each lower premolar and molar, and in the basic pattern of each tooth crown. Moreover, the relations of the cusps of the upper and lower cheek teeth in occlusion reveal the extraordinary degree of correspondence between *Dryopithecus* and Le Moustier in all fundamental characters, so that it may be stated as a fact that with regard to the dentition, the primitive anthropoid is far nearer to the primitive human than it is to the Eocene primate *Pronycticebus*.[2] Thus the various detailed differences between the Mousterian incisors, premolars, molars, and those of *Dryopithecus*, as described in the papers cited, are far less profound than the differences between the corresponding teeth of the tapir and of the horse.

All these facts assuredly add weight to the argument that the kinship of man to the chimpanzee is far closer than that of the tapir to the horse, and that the separation of the first pair was a much later event than the separation of the second pair.

[1] Gregory, William K., 1916, "Studies on the Evolution of the Primates," *Bull. Amer. Mus. Nat. Hist.*, XXIV., Art. XIX., pp. 239–355.

[2] Gregory, William K., 1926, "Palæontology of the Human Dentition," *Amer. Journ. Phys. Anthrop.*, Vol. IX., No. 4, pp. 420–422.

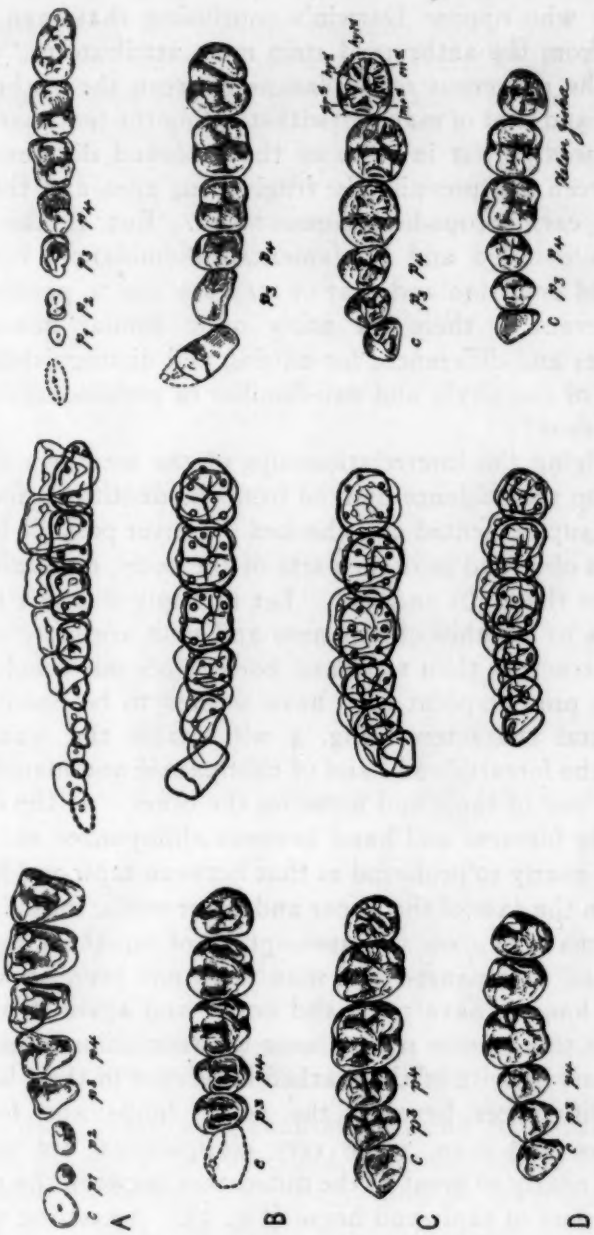

FIG. 3.—Upper and lower cheek teeth of *A*, Eocene primate, *Pronycticebus*; *B*, *Dryopithecus*; *C*, Le Moustier; *D*, *Homo sapiens*.

Those who oppose Darwin's conclusion that man is an offshoot from the anthropoid stem must attribute to "parallelism" the numerous resemblances between the anthropoid dentition and that of man, notwithstanding the fact that these resemblances persist in spite of the profound differences in diet between the prevailingly frugivorous apes and the prevailingly carnivorous-herbivorous man. But if the very numerous detailed and fundamental resemblances between anthropoid dentition and that of man are due to parallelism, what warrant is there for using quite similar dental resemblances and differences for uniting and distinguishing the members of the phyla and sub-families of perissodactyls and proboscideans?

In judging the interrelationships of the members of any large group the evidence derived from the dentition should of course be supplemented and checked wherever possible by the characters observed in other parts of the body, especially the skeleton of the limbs and feet. Let us apply this test to our inquiry as to whether chimpanzee and man are more nearly allied in structure than tapir and horse, since our conclusions up to the present point may have seemed to be based only upon dental characters. Fig. 4 will enable the reader to compare the forearm and hand of chimpanzee and man on the one hand and of tapir and horse on the other. Is the difference in the forearm and hand between chimpanzee and man anywhere nearly so profound as that between tapir and horse? Here, as in the case of the upper and lower teeth, the evidence suggests that even on the assumption of equal changes in equal times, chimpanzee and man have not been separated nearly so long as have tapir and horse, and again it will be noted that this greater resemblance between chimpanzee and man persists in spite of the marked difference in their habits.

The differences between the pelvic limbs and feet of chimpanzee and man, while very conspicuous, are on the whole not nearly so great as the differences between the pelvic limbs and feet of tapir and horse (Fig. 5). According to the well established principle of adaptive radiation, after the

descendants of an ancient common stock pass from the ancestral life-zone to a new one, their whole locomotor skeleton becomes adapted to the new mode of life. It is well

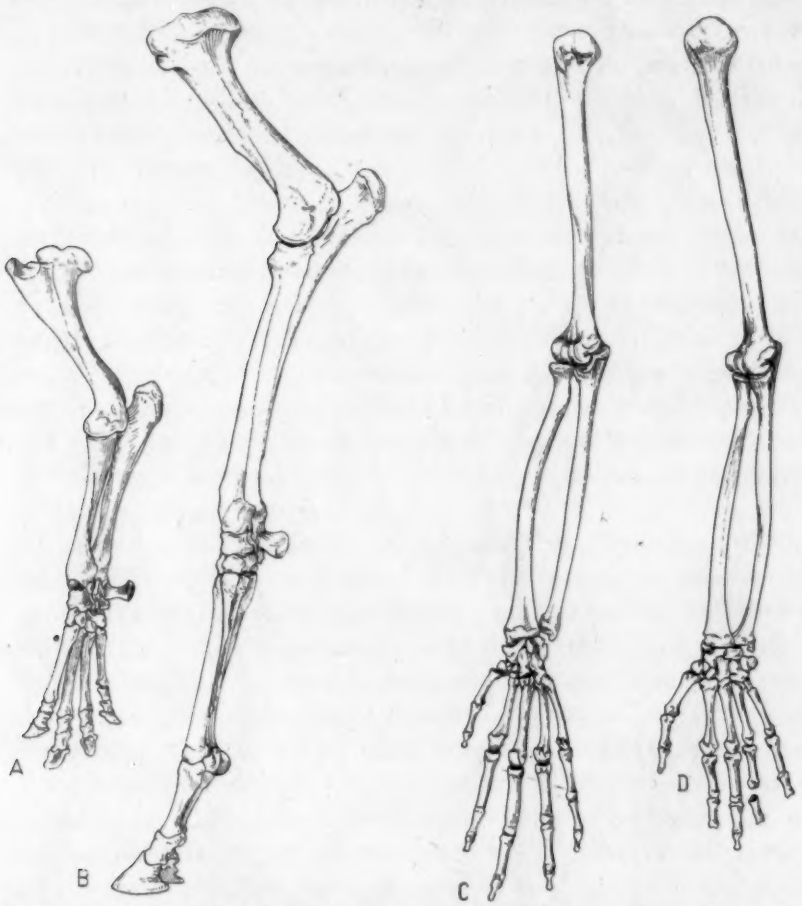

FIG. 4.—Skeleton of right pectoral limb of *A*, Tapir; *B*, Horse; *C*, Chimpanzee; *D*, *Homo sapiens* (Veddah). *C* and *D* after Sarasin.

known that these new adaptations tend to cover up and obscure the characters inherited from the older environment. The chimpanzee and man, living now in different environments and contrasting widely with each other in their modes of locomotion, show the maximum contrast in their pelvic

limbs, whereas the horse and the tapir, continuing to use their limbs in much the same way as did their remote common ancestors, differ from each other chiefly in the fact that in the

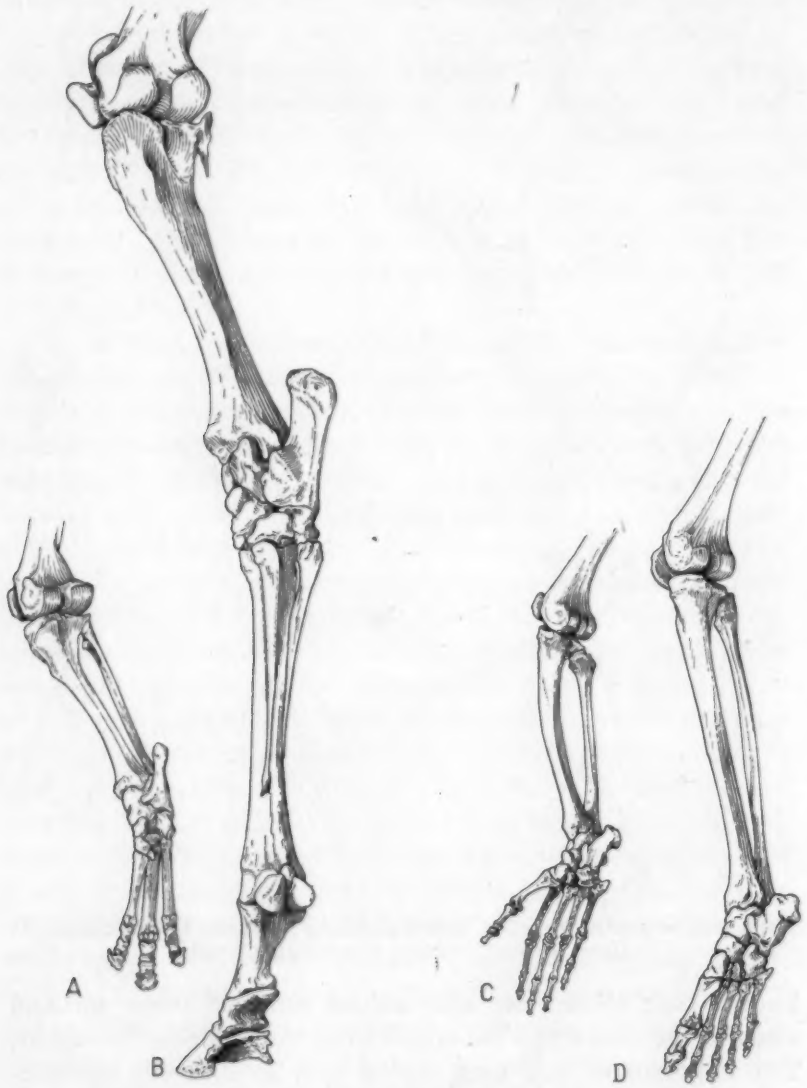

FIG. 5.—Skeleton of right pelvic limb of *A*, Tapir; *B*, Horse; *C*, Chimpanzee; *D*, *Homo sapiens*.

horse tendencies toward centralizing the axis of the foot, which are already clearly visible in the tapir, have been carried to the extreme. And it will be shown presently that in spite of the striking difference of the human foot from that of the chimpanzee, its origin is perfectly explicable on the hypothesis that it has been derived from the foot of a primitive anthropoid type by a definite change of function involved in the abandonment of arboreal life and the assumption of bipedal running habits.

The striking difference between the foot of the anthropoids and that of man has led Sir Ray Lankester and others to regard the evolutionary gap between anthropoids and man as equally profound, and has been the principal objection to Darwin's theory of the origin of man. This brings us to the very kernel of the whole question, namely, was man's place in nature correctly determined by Darwin, Huxley and Haeckel; is he still definitely the next of kin to the anthropoid stock, or does he represent an entirely independent group of unknown origin and relationships?

In his recent articles on the origin of man Professor Osborn rules the Simiidæ or apes out of the line of ascent to man on the ground that they have ape brains and ape minds, that they have degenerate thumbs and limbs adapted for acrobatic life in the trees, that they walk on all fours and have grasping hind feet. He also points out that the *Pithecanthropus* had a definitely human brain, that the Neanderthal man had descended from thousands of generations of upright-walking men, that the modern human foot shows no evidence of derivation from a proto-anthropoid type, and that when men climb trees, they climb in an awkward human way which he holds is quite unlike the method adopted by the apes.

Anti-evolutionists of all schools are doubtless rejoicing in the fact that Professor Osborn has repudiated man's descent from apes and has brought forward with all the authority of his name some of the very points which they have long been stressing. But their exuberance will be dampened somewhat when they realize that Professor Osborn, like Professor Wood

Jones, separates man from the anthropoids only in order to derive him eventually from a *far lower* branch of the primate stock. Professor Osborn goes even lower down the tree than does Professor Wood Jones, since he suggests the possibility of man's derivation from some form like *Notharctus* of the Lower and Middle Eocene of North America.

Out of all the confusing tangle of resemblances and differences between men and apes, opponents of Darwin's solution of the problem have regularly seized upon a few of the more evident dissimilitudes, to which they have given wide publicity. But they have consistently ignored or depreciated the mass of positive marks of kinship visible in the very early fetal stages of apes and men, as well as in adult anatomy and in profound physiological reactions. Even the older literature contained a great deposit of evidence which should long ago have settled the question definitely. In the past few decades the patient and well-tested observations of Schwalbe, Dubois, Keith, Elliot Smith, Hrdlička, Huntington, Tilney, McGregor, Wingate Todd, Wilder, Sonntag, Morton and many others on the anatomical side, and of Nuttall, Uhlenhuth, Landsteiner and others in the physiological field, all confirm Darwin's view and no other; but although impregnable, this evidence remains virtually unnoticed by opponents of Darwin's conclusion.

This evidence as to man's kinship with the anthropoids is always weakened by being cited in small quantities, since its logical value lies in its cumulative weight. Nor is a mere summation of separately listed characters common to man and anthropoid satisfactory, since opinions would differ as to whether a complex of functionally related characters should be entered as one or many. The defender of the Darwinian view is truly at a disadvantage precisely because his evidence is too extensive to be fully exhibited to his opponents. On the other hand, some who oppose Darwin's conclusion as to the derivation of man from the anthropoid stem imagine that they have raised serious objections to it, if they can bring forward even a few characters wherein modern man differs from

modern apes. Thus Professor Wood Jones in a recent paper [1] states that on the inside of the human skull "the presphenoid bone and the mesethmoid articulate together over a wide area. In the Monkeys and the Anthropoid Apes (again with the exception of the Orang) the mesethmoid is excluded from contact with the presphenoid by an ingrowth of the frontals between the two." However this may be in the material examined by Professor Wood Jones, the specimens available in the American Museum of Natural History show that in all the young gorillas and chimpanzees at hand the ingrowths of the frontals from the sides do not disrupt the contact between the mesethmoid and the presphenoid but either overlap it slightly (Plate I) or restrict it to a narrow isthmus. Professor Wood Jones thus permits this supposed difference to outweigh the fundamental identity in plan between the young gorilla skull and the young human skull, and does not even see that the fronto-presphenoid contact of the adult gorilla is a superficial contact which conceals, but does not replace, the sphenoid-ethmoid contact beneath it.

Professor Wood Jones also makes far too much of the fact that in the "Monkeys and Apes (except, at times, in the Orang) the temporal bone meets the frontal, and the alisphenoid fails to meet the parietal," but he does not mention the fact that in the gibbon the fronto-temporal contact is normally lacking (Sonntag), while the great wing of the sphenoid meets the parietal bone as in man. In a certain very young gorilla skull (A. M. N. H. No. 54336) the fronto-temporal contact is far smaller than in the adult. Deniker [2] records that in certain human skulls the fronto-temporal contact occurs, while in one side of one gorilla skull the parieto-temporal contact was found. According to E. Fischer and Th. Mollison, the fronto-temporal contact occurs with increasing frequency as we pass from the European race (1.5 per cent.) to the Negros (11.8 per cent.) to the Gibbon (13.7 per cent.), Orang (33.6 per cent.), Chimpanzee (77.0 per cent.) and Gorilla (100 per cent.). They also suggest that the

[1] "The Ancestry of Man," Brisbane, 1923, pp. 26, 27.
[2] "Recherches sur les Singes Anthropoids," p. 62.

PLATE I.—Inner surface of the forehead of *A*, infant gorilla; *B*, human child, showing the relations of the frontal bones to the mesethmoid.

occurrence of the fronto-temporal contact is dependent to a high degree upon the relative size of the brain. The more the skull roof is lifted up, the more the frontal and temporal bone will be pressed away from each other and the more rarely will they unite in a suture. Thus as the human skull in many respects represents an infantilized anthropoid type, either the loss of a small fronto-temporal contact, or the retention of a primitive spheno-parietal contact, is clearly of no great morphological importance.

Obviously there are differences, and many of them, between man and the modern apes. If it were not so, there would either be no apes or no men and the problem would not now be under discussion. The first vital question is, are the characters, many or few, that are common to men and apes, due to inheritance from a more remote common stock, or are they due to parallelism? If they are due to the latter, then, if it still be admitted that man belongs in the order Primates, to what group other than the great apes is he most nearly related, by what steps has he diverged from that group, in what part of the world may we search for his ancestors and how shall we recognize such ancestors when we find them?

These questions in turn are quite obviously tied up with the general problem of the classification and geographic distribution of the families and subfamilies of the tree-shrews, lemurs, South American monkeys, Old World monkeys, anthropoids and men, both recent and fossil. Since 1910 I have published a series of carefully worked out analyses of these problems, in which the Darwinian view of man's origin has been steadily upheld. The opponents of this view have not met the issues discussed in these papers. They have not attempted a direct refutation of my arguments; they have not shown wherein I erred either as to facts or principles. They have simply made a flank attack by citing a list of admitted differences between men and anthropoids.

In the papers cited above the evidence has been presented for the following outline of the history of the primates. By Basal Eocene times the primates were already in process of

differentiation from the tree-shrew stock, which all authorities now regard as structurally ancestral to the primates. By Lower Eocene times we distinguish in both Europe and North America, first, an already highly differentiated tarsioid stock, one of the branches of which culminated in the modern *Tarsius*, and secondly, the *Notharctus-Adapis* group. The adapine branch of this family was peculiar to Europe and, according to my analysis of its characters, was quite closely related to the recent and Pleistocene lemurs of Madagascar. The South American monkeys seem on the whole to be an offshoot from the base of the tarsioid stock, rather than a descendant of *Notharctus*.

The structure of the hind foot is definitely known in representatives of the Eocene families Plesiadapidæ, Anaptomorphidæ, Notharctidæ, Adapidæ, and in each and all of these, as well as in all known recent and fossil lemurs, South American monkeys, Old World monkeys and apes, the hind foot has a widely divergent great toe, with a flat nail. Thus all the known palæontologic, zoölogic and embryologic evidence supports the conclusion that from their first appearance the Primates as an order were thoroughly arboreal and that the terrestrial habits of the baboons and of man are a later acquisition. The early human fetus also retains the marked divergence of the great toe and even the adult human hallux retains the broad flat nail like those of arboreal Primates.

None of the lower groups—lemurs, tarsioids, South American monkeys—approach man except in obviously parallel features. They all stand on a distinctly lower plane and differ in many trenchant characters which are discussed in the papers cited.

The Old World series makes its appearance in the Lower Oligocene of Egypt. The most primitive known form, *Parapithecus*, as its describer, the distinguished palæontologist Max Schlosser, fully perceived, is structurally intermediate between the stem of the tarsioids on the one hand, and the whole Old World series on the other. Although only the lower jaw is known, this highly important form must have had

the shortened face and the swollen braincase, and probably the large eyes, of the small insectivorous tarsioids. Side by side with it is the oldest of the true anthropoids, the lower jaw of *Propliopithecus haeckeli*. This has the dental formula common to the Old World monkeys, anthropoids and man, but its mandible is deepened like that of the frugivorous anthropoids and its molars already foreshadow the *Dryopithecus* pattern of the anthropoids and man. It was plainly akin to the gibbons but smaller and more primitive. The eminent anthropologist Sergi has selected *Propliopithecus* as an ideal ancestor of man, but there is reason to believe that the human stem did not split off so far down the line. The modern gibbons have become specialized in the extreme length of their limbs and in the sabrelike form of their upper canine teeth, but they retain the ischial tuberosities and other primitive features that ally them both with the Old World monkeys and with the anthropoids. According to Sir Arthur Keith's illuminating researches, the modern gibbons have already effected the profound readjustments of the viscera necessary for the upright posture habitually adopted by the gibbon; this ape is no longer an arboreal quadruped but a brachiating, upright-moving anthropoid; its visceral arrangements are actually far nearer to man than to the lowest of the primates (Keith).

In the Middle and Upper Miocene and Pliocene of India and in the Upper Miocene and Pliocene of Europe there was a wide deployment of the anthropoid group, known, it is true, chiefly from teeth and jaws, some of which approach the modern genera. All develop the "*Dryopithecus* pattern" of the molars, the remnants of which are so clearly seen in man.

In the lower primates, as Dr. Morton[1] has shown, the principal axis of weight primitively passes through the third or middle digit of the hind foot; such animals run upon the branches like arboreal quadrupeds. In the anthropoids the main axis of weight is shifted toward the inner side of the foot and passes between the great toe and the first digit, in adaptation to the grasping habit of the foot. In the second-

[1] 1924. "Evolution of the Human Foot," *Am. Journ. Phys. Anthrop.*, Vol. VII., No. 1, pp. 10–13.

arily terrestrial *Gorilla beringei* the heel is broadened, the whole foot is pressed flat upon the ground, the toes are relatively shorter and the great toe relatively larger. In the human fetus of the ninth week the great toe is enlarged and strongly divergent (unlike the small, slightly divergent first digit of primitive Eocene placentals), the main axis passes between the first and second digit, the other toes are relatively longer than in the adult and the whole foot recalls the anthropoid condition and differs widely from the adult foot, which doubtless for more than a million years has become thoroughly adapted exclusively for terrestrial locomotion.

Sir Ray Lankester, knowing well only the more arboreal foot of *Gorilla gorilla*, endeavored to cast doubt on the evidence afforded by the more terrestrial foot of *Gorilla beringei*, and clings to the belief that the peculiar construction of the human foot still constitutes a bar to the derivation of man from the anthropoid stem. Professor Adolph Schultz,[1] on the other hand, has shown that during the course of ontogeny the human great toe is at first distinctly more anthropoid and partly turned toward the other toes, but that as development proceeds it becomes twisted on its long axis so as to face downward, and is also drawn in toward the other toes. "In his paper on the evolution of the primates, Gregory (1916) enumerated the following conditions which would be necessary in order to transform a gorilla-like foot into a human foot: 'To increase the length of the hallux, to adduct it and rotate it on its own axis so that its plantar surface shall be applied to the ground instead of facing toward the other digits. Next it would be necessary to shorten still further the phalanges and to narrow the whole foot; that is, to make all the digits parallel instead of divergent, and the whole foot must be 'pronated' or made to face downward rather than inward.' In summarizing some of the conditions of development of the human foot, as given here and in the author's last paper (1923), one might say that in order to transform an early fetal foot into the adult foot a number of changes are

[1] Schultz, Adolph H., 1924, "Growth Studies on Primates Bearing upon Man's Evolution," *Amer. Journ. Phys. Anthrop.*, Vol. VII., No. 2, p. 162.

necessary; to enumerate these would be to repeat, word for word, the points mentioned by Gregory for the hypothetical transformation of the foot of a gorilla into that of man."

The very fact that the great toe is the dominant one is strong evidence for the view that the human foot has been derived from an anthropoid with a grasping great toe, for no other known primates afford such a favorable starting point for the human condition. Moreover, the musculature of the human foot shows convincing evidence of special relationship to the anthropoid foot, as well understood from the time of Huxley. And on the anthropoid side, the presence of a peroneus tertius muscle in the gorilla, as a variant similar to one of the variants of this muscle in man, tells in the same direction.

In view of all this and of the fact that man is tied by so many other bonds to an order which was thoroughly arboreal in its first stages, the burden of proof would seem to lie upon anyone who prefers to maintain that the construction of the human foot constitutes a serious obstacle to the derivation of man from a pre-anthropoid stem.

The reduction of the thumb in anthropoids is cited by Professor Osborn as ruling them out from the line of human ascent. But the remarkable feature is, not that anthropoids should have the thumb reduced, but that they should have a thumb at all. No one can doubt, after inspection of Fig. 4, that the chimpanzee has a true hand and not a mere forefoot like that of quadrupedal mammals. The fundamental resemblance to the human hand extends to such details as the arrangement of the extensors of the hand, as described by Professor J. E. V. Boas,[1] and in the light of his results, who can doubt that the thumb of man has not only increased in size but also improved its ability to oppose the other digits. Professor Schultz states: "In early fetal life the free thumb branches from the palm immediately at the base of the index finger. In the course of growth this place of branching shifts proximally to a

[1] Boas, J. E. V., 1919, "Einige Bemerkungen über die Hand des Menschen," *Det. Kgl. Danske Videnskabernes Selskab. Biologiske Meddelelser*, II., 1, pp. 1–32, Figs. 1–25.

place nearer the wrist. This ontogenetic migration is most pronounced in orang-utan and gibbon, somewhat less in other anthropoids and man, as well as in most Old World monkeys, while in the large majority of the American monkeys no such shifting takes place at all, the thumb persisting in its typically fetal position throughout life. This movement of the thumb, away from the other fingers, in the catarrhine hand has greatly facilitated the opposability of the thumb. In platyrrhines this digit is either not opposable at all, as in the marmosets, or, as in the Cebidæ, only to a much lesser degree than in the African and Asiatic primates. The opposability of the thumb, which was and is of greatest importance for the evolution of man, was not yet a feature of the original primate hand. It is significant, but not surprising, therefore, to find that this condition is still lacking in the human embryo. Not only does the embryonic thumb branch at a place unfavorable for effective opposability, but it is not yet rotated around its longitudinal axis to face the other digits, as is clearly shown by the diagrams in figure 11." [1]

Thus while the human thumb passed from a stage where it is more nearly parallel to the other digits to a stage where it can oppose them, the great toe developed in the opposite direction from a stage where it tends to face the other digits to a stage in which it is parallel with them.

It may well be true that apes have ape minds and ape brains, adapted to life in the forest, whereas the Dawn Men (*Pithecanthropus*, Piltdown, etc.) had definitely human minds and brains which for thousands of generations had adapted them for life on the plains. But this only establishes the fact that apes and men are different and have been different for a very long period of time. It does not throw any light on the questions whether or not man is an offshoot from the anthropoid stem, what that stem was like, and during what geologic epoch the separation occurred. If we hold with the anti-evolutionists that the human mind has not evolved out of any animal mind, then the labors of comparative psychologists and

[1] "Fetal Growth of Man and Other Primates," 1926, *Quart. Review Biol.*, Vol. 1, No. 4, p. 495.

comparative neurologists are in vain. But if we accept man as a member of the order Primates and a derivative of some form of pre-human primate, then the evidence of comparative psychology and of comparative neurology must be taken into account, unless one resolutely refuses to give value to any except palæontologic evidence. What then is the testimony of comparative psychology and comparative neurology? From the labors of Koehler, Kohts and Yerkes it may be asserted that, although far below man in mental ability, the anthropoids are unquestionably much nearer to man than are any of the lower animals of which the mentality has been carefully tested. Indeed, Yerkes, a most cautious and conscientious investigator, finds in the anthropoids more than the rudiments of human thinking. And on the side of the "Dawn Men," Dubois, Elliot Smith, Hunter, Tilney, McGregor, point out the distinctly inferior development of the *Pithecanthropus* brain as compared with the brain of modern man, in respect to the filling out of the critical prefrontal, parietal and temporal and occipital areas, which from clinical research and other lines of evidence are believed to be the seat of the higher mental faculties. The *Pithecanthropus* mentality then, while coming within the limits of the human family, was, so far as the braincast indicates, by no means lacking in lowly traits.

Opponents of the Darwinian view should never refer to the comparison of the brains of apes and man, for there is no division of morphology that so fully testifies to the relatively close kinship of man to the gorilla and the chimpanzee, as the field of comparative neurology. The utmost efforts of anti-evolutionists have only brought into clearer relief the basic correspondence in all parts, not only of the cerebral cortex, but of the brain stem of gorilla and man. The anthropoid brain, according to the well seasoned conclusions of Elliot Smith and Tilney, carries the line of evolution from the lower primates to a definitely subhuman stage. Doubtless the orang is a side specialization in some features but the gorilla brain stands especially near to the primitive human brain.

All of this is in full concordance with the evidence from all the other sources, that the human stock derived a rich heritage from arboreal ancestors, which, while fully erect in posture, avoided the extreme specializations of any of the existing apes and abandoned the trees before the thumb was greatly reduced or before the body was as heavy as that of the gorilla (Morton).

The principal objection to deriving man from a point far down the primate tree, from such a form as the Eocene *Notharctus*, is precisely the lack at that early stage of the very numerous characters which connect the human stock with that of the anthropoids.

If the numerous converging lines of evidence for Darwin's view carry conviction to our minds, the next question is, when and where did the separation take place? As to the time when, the separation must plainly antedate Mid-Pliocene times, but the numerous millions of years of the Lower Pliocene and Upper and Middle Miocene, during the long period when the anthropoid radiation was in full swing, would seem to allow sufficient time for the accelerated evolution which may have taken place when a marked change in food habits, consequent upon the invasion of the plains, conditioned a higher instability in the endocrine system. Bolk, Keith and others have pointed out the far-reaching effects of the progressive retardation of the period of maturity in the progressive human line, the opportunity thus afforded for enormous increase in brain capacity and a profound remodelling of the entire organism.

If man is not a derivative of the primitive anthropoid stock, and if he is at the same time to be classed in the order of Primates, from what other group did he spring? The tailed monkeys of the Old World exhibit a wide deployment of subfamilies, genera and species, but all of them are sharply distinguished from the anthropoids and man by the character of their cheek teeth, which definitely place them as a specialized side line. They also retain the primitive condition of the hind feet, in which the main axis of weight passes through the third digit, whereas in the anthropoids and man it has been

shifted to the inner side of the foot. Professor Boule has suggested that perhaps man separated from the Old World stock at a period when the relative lengths of the limb segments were more as they are in the cynomorph monkeys, that is, before the lengthening of the arms and the shortening of the legs in the modern anthropoid group; but in view of the profound agreement of man with the anthropoids in brain characters, blood tests and fetal development, a definitely pre-anthropoid derivation of man lacks substantial evidence. Still less claim have the South American monkeys to close relationship with man, and the arguments of Ameghino in this direction have been definitely refuted by Blüntschli and others.

The theory that man may be taken off from the base of the tarsioid stock without embarrassing relationships with the anthropoids must needs beg the question by dismissing the structural agreements between man and anthropoids as parallelisms. In view of all this, if man is not derived from the anthropoid stock, why should his early primate ancestors be sought in the Miocene and Pliocene of Asia, where the Primate order has, so far as known, been represented only by the varied monkeys and anthropoids? If on the other hand, man is so derived, there is added reason to search for his early representatives in some region of open plains, not too far removed from the ancestral forests of the conservative anthropoids.

Thus, as to the place where the human stock began to separate from the primitive chimpanzee-gorilla group, we can reasonably expect to find that it was somewhere within the known range of the anthropoid group in the Miocene and Pliocene, that is, somewhere between Western Europe and Eastern Asia. Here we may refer to the excellent analysis of this question by Grabau and Black, who indicate the region of the Tarim desert as the most likely place in which to renew the search.

SEA-LEVEL SURFACES AND THE PROBLEM OF COASTAL SUBSIDENCE

By DOUGLAS JOHNSON and ELIZABETH WINTER

(*Read April 30, 1927*)

I. The High Tide Surface

In papers [1] previously published, the senior author has shown that many phenomena commonly attributed to progressive coastal subsidence in modern times may more reasonably be explained as consequences of local fluctuations in the high-tide level due to changes in the form of the shoreline. It is well known that an embayment in the coast like that of the Bay of Fundy, broadly open at its mouth but narrowing inward toward its head, causes each entering tidal wave to rise higher and higher as it advances, until its crest is well above the level of high tide in the ocean. Equally well known is the fact that in a broad bay connecting with the ocean by a narrow inlet only, the tides in the bay cannot rise as high as high tide in the ocean, for the simple reason that water cannot pour through a narrow inlet fast enough to fill the bay before the tide outside has begun to fall again. Thus it happens that in some embayments of the coast high tide is abnormally high, in others abnormally low.

It is not so widely realized, outside the ranks of those especially concerned with tidal problems, that other irregularities of the coast less clearly marked than the extreme cases

[1] "The Supposed Recent Subsidence of the Massachusetts and New Jersey Coasts," Douglas Johnson, *Science*, N. S., Vol. XXXII., pp. 721–723, November, 1910; "Botanical Evidence of Coastal Subsidence," Douglas Johnson, *Science*, N. S., Vol. XXXIII., pp. 300–302, Feb., 1911; "Fixité de la côte Atlantique de l'Amérique du Nord," Douglas Johnson, *Annales de Géographie*, tome XXI., 193–212, Mai, 1912; "Botanical Phenomena and the Problem of Coastal Subsidence," Douglas Johnson, *Botanical Gazette*, LVI., 449–468, December, 1913.

mentioned above, exert an appreciable influence upon the height of the tide. Some years ago the senior author, with the aid of a number of graduate students in Columbia University, observed the heights reached by the same high-tide wave at widely separated points in various channels and bays about the City of New York. At each point the maximum height of the tide was measured with reference to a fixed mark, and these marks were then connected with the precise level net of the New York region surveyed by Mr. Frederick W. Koop, Engineer of the Board of Estimate and Apportionment of New York City. The results are given below:

Station	Observer	Elevation of Crest of Tide Wave (May 21, A.M., 1913) Above Mean Sea Level in Feet
1. Mariners Harbor, Staten Island	Charles R. Fettke	2.364
2. Snug Harbor	Warren S. Smith	2.200
3. Fort Hamilton tide gauge, Long Island	G. S. Kearney	2.180
4. Ordnance Dock, Governor's Island	F. F. Hinze, Jr.	2.118
5. Foot of East 84th St. East River	E. L. Bruce and D. W. Johnson	2.491
6. Steinway, East River	F. M. Van Tuyl	3.085
7. College Point, East River	Ralph W. Howell	3.519
8. Willet's Point, East River	Stopford Brunton	3.619
9. Foot of Dyckman St., Hudson River	G. J. Mitchell	1.819

It will appear from the foregoing figures that if we were to pass an imaginary plane through all the points reached by the

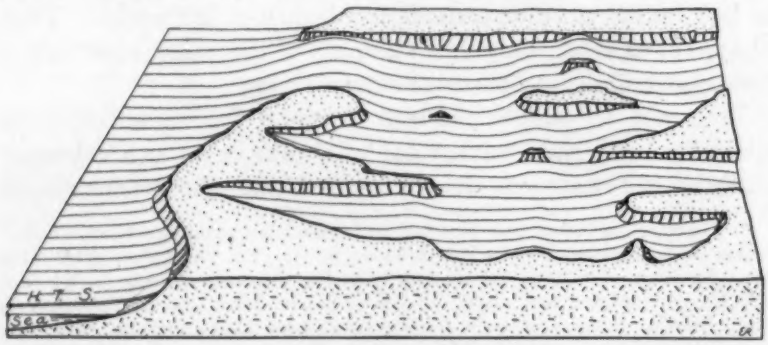

Fig. 1.—The form of an imaginary surface—"the high-tide surface"—passing through all points reached by the crest of a high-tide wave on an irregular coast. The irregularities of the high-tide surface are purposely exaggerated.

high tide, it would be a badly warped plane, rising and falling in different areas, the undulations having vertical dimensions of from one to two feet in the New York area. We may call such an imaginary warped plane "the high-tide surface," Fig. 1. Obviously the high-tide surface on the coast of Maine, where indentations are numerous and large and the tidal range is great, must be an extremely irregular surface, sloping upward in funnel-shaped bays with broad mouths, dropping abruptly downward where large bays connect with the ocean by narrow inlets, and rising and falling alternately in tortuous channels of variable form surrounding islands and peninsulas.

It must fully be appreciated that the high-tide surface does not correspond with any real water surface, let alone with the surface of the tidal wave itself.[1] This is apparent from Fig. 2,

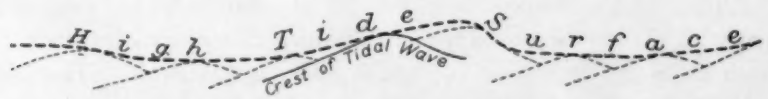

FIG. 2.—Diagram to show that the high-tide surface (heavy broken line) is imaginary and does not correspond to any real water surface, whereas the tidal-wave surface is a true water surface occupying successively the positions shown (solid and light broken lines). Elevations much exaggerated.

where the solid line represents a cross section of a high-tide wave at any given instant, the light broken lines earlier and later positions of the same wave as it rises or falls in its progress along an irregular coast, while the heavy broken line passes through all points touched by the crest of the wave during its advance. Both the solid line and the light broken lines correspond to actual water surfaces which momentarily existed when the wave was successively in the positions they indicate; but the surface of the sea never, not even for an instant, possessed the form indicated by the heavy broken line. So also the warped surface of Fig. 1 is an imaginary surface constructed by connecting all the points touched by the crest of the high-tide wave during its fleeting passage.

[1] We may ignore for the present the close approach to coincidence between the imaginary high-tide surface and the water surface of a bay with narrow inlet, in which currents are mainly hydraulic and little of the true tidal oscillation is felt.

But if the high-tide surface itself is an imaginary thing, its importance is very real. Below the high-tide surface salt water is found, not continuously but intermittently. As such an intermittent wetting by salt water is harmful to fresh water vegetation, most of the plants along our shores cannot grow below the high-tide surface, although they may grow down to it. Thus it is the imaginary high-tide surface, not mean sea level or low-tide level, which is the most obvious and significant plane separating the domain of the sea from the domain of the land. On the shores of nearly enclosed bays, trees, shrubs, and upland grasses may grow well down below the level of high tide in the adjacent ocean because, as is indicated above, the high-tide surface in such bays is abnormally low. On the other hand, fresh water vegetation along the shores of the Bay of Fundy and other bays of similar form, does not grow as far down the slopes as it does elsewhere, because in such bays the high-tide surface is abnormally high. The line separating upland from marine vegetation along the shores falls and rises as the high-tide surface falls and rises. This line is the trace of the high-tide surface against the slopes of the land, and is a real and visible thing. Practically, its variable elevation is not often noticed, because as a rule the differences of altitude are to be measured in feet or inches, whereas parts of the line having distinctly different altitudes are separated by distance measurable in rods or even miles. Furthermore, other factors, such as the degree of exposure to storm waves, affect the level to which fresh vegetation will grow down the slopes of the land.

But there come times when the trace of the high-tide surface against the shores forces itself upon our attention. This is when some change in the form of the shoreline causes a local rise in the high-tide surface. Since the irregularities of the warped or undulating high-tide surface are directly dependent upon irregularities of the shore, it is obvious that changes in shore form must be accompanied by changes in the form of the high-tide surface. If an inlet is broadened, so that tides may enter a bay more freely than before, the high-tide

surface in the bay will rise. If a sand spit partially closes a funnel-shaped bay which formerly was open wide to the ocean, the high-tide surface in the bay will fall. So also will other changes in shore form, inlet width, or channel depth cause changes in the elevation of the extremely sensitive high-tide

FIG. 3.—Trees bordering the shore of the North River embayment near Scituate, Massachusetts, killed by a local rise of the high-tide surface due to a breach in the bay-mouth bar effected by the "Portland Storm" of 1898.

surface. If the changes in shore form take place suddenly, the resulting changes in the high-tide surface may accurately be dated, as witness the sudden rise of the high-tide surface in the North River embayment near Scituate, Massachusetts, Fig. 3, following a breach in the bay-mouth bar caused by the "Portland Storm" of *1898*.[1] If the changes take place slowly, the imperceptible changes in the level of the high-tide surface are less apt to attract attention; and if results of the

[1] Douglas Johnson, "New England-Acadian Shoreline," 591 pages, New York, 1925, pp. 545–548.

changed tidal range are later encountered, correlation with their true cause is not easily made.

There can be no doubt that changes in elevation of the high-tide surface are constantly taking place along the Atlantic coast of North America. The irregularities of the shore, the wide distribution of unconsolidated glacial débris or coastal-plain sediments easily attacked by waves, the extensive development of bars, spits, inlets, and other shore forms subject to rapid changes in form, position and size, coupled with the moderate to large tidal range, assure exceptional opportunities for continual fluctuations of the high-tide surface. So long as the change involves a lowering of this surface, even if sudden, the effects are not readily noticeable; for in the very nature of the case the extension of fresh-water vegetation into areas relinquished by the sea is gradual and imperceptible. But when the high-tide surface suddenly rises, the effect is quickly apparent. Trees and shrubs are killed, and their skeletons remain as obvious proofs of the change, Fig. 3. It is not surprising, therefore, that observers along the coast should be impressed with the abundant evidence of salt water rising higher on the lands; and should fail to note evidence, perhaps equally widespread, but certainly less readily detected, of lowerings of salt water levels. It is this apparent predominance of sea encroachment which lends a fictitious strength to the theory of a continuing coastal subsidence; for if the evidences of a lowering high-tide surface were as conspicuous as those of a rising one, there can be little doubt that the local and temporary nature of both changes would more readily be appreciated.

II. The Mean Sea-level Surface

It is evident from what has been said above, that the high-tide surface has no value as a plane of reference for determining general changes in the level of land or sea. As a cause of fictitious appearances of coastal subsidence, the high-tide surface is an important object of study; but for the measurement of real changes of land level or general changes in sea

level, we require a datum plane less irregular in form and more stable in position. Mean sea level suggests itself as the most desirable plane of reference for the purpose in question.

One might suppose that however much the high-tide surface may vary, due to varying amplitudes of tidal oscillations, the mean plane above and below which the tides rise and fall would be a perfectly "level" surface (*i.e.*, possessing the true curvature of the earth's surface form, exclusive of the lands rising above sea level), and that this level surface would remain constant and immovable in position. A little inquiry will, however, soon convince anyone that the sea-level surface is by no means level; and may raise doubts in his mind as to whether it is always constant in position.

An ocean current directed against a land mass may bank the waters up against the coast to produce there an abnormally high sea level. On-shore winds raise the ocean level against a land, while offshore winds lower it. Variations in barometric pressure, heavy rainfall, the influx of great volumes of river water, and excessive evaporation are some of the other factors which may cause the mean sea-level surface to be raised or lowered in respect to the normal sea-level plane. These distortions of the sea-level surface are neither so slight in amount nor so ephemeral in character that they can be neglected when one is considering the mean position of the sea surface as determined by measurements extending over long periods of time. Departures from the normal position of sea level due to the causes above cited may often be measured in inches, and sometimes in feet. Some of these departures are essentially permanent; others are seasonal in some places but permanent in others. We must therefore imagine the mean sea-level plane as a warped or undulating surface, rising above the average level in some places, sinking below it in others. Compared with the irregular high-tide surface, the undulating mean sea-level surface shows smaller and far more gradual changes in elevation, usually distributed over very broad areas. Like the high-tide surface, the mean sea-level surface is imaginary and never corresponds with an actual water sur-

face over any great area. Its position cannot, therefore, be directly observed, but must be determined by long series of measurements of the always restless surface of the sea.

There are serious difficulties involved in the determination of mean sea level which make its employment as a plane of reference more complicated than is generally realized. Those who have cited apparent changes in mean sea level as proofs of coastal subsidence, have often failed to appreciate the nature and extent of these difficulties. Some have assumed the accuracy of a mean sea-level observation made by a trustworthy organization or individual, without enquiring into the nature of the observations made, or the degree of precision required for the purposes then in view. It has frequently been further assumed that a few months, or at most a year or two, of careful tidal observations would be sufficient to fix the plane of mean sea level. Some who admit the inadmissibility of the high-tide surface as a plane of reference, have assumed that however much that surface may vary in elevation with changes in the form of the shore, the mean sea-level surface is not subject to such fluctuations. The first two assumptions certainly are not justified; and in our opinion, the third is equally untenable.

To ascertain the mean level of the sea, an imaginary plane, the position of which is not indicated even approximately by any features found along the shore, one must measure the rise and fall of the water caused by tidal and other forces, and average the results according to well determined methods. The height of the water surface must be recorded hour by hour, since half-tide level, found by averaging the heights of high and low waters only, is not the same as mean sea level, and may lie either above or below that level. For reliable measurements, automatic or self-registering tide gauges are employed; but the proper installation and operation of these gauges is by no means as simple a matter as might be supposed. If the tidal range is great and the shore sloping, a gauge installed near the shore will record the upper part of the tidal oscillation only, ebb tide carrying the water far out beyond

and below the bottom of the gauge. Installation farther out is often difficult, for storm waves, currents, or ice are apt to damage the gauge if it be located in an exposed position. The disturbing effects of waves upon the tidal curve must be "damped out" by placing the gauge in a sheltered position, often in a pipe or well into which the water is admitted by another pipe extending seaward far enough to have its open end well below the level of serious wave agitation. In cold climates, the water in the well or pipe must be kept from freezing, and for this purpose oil must be introduced into the pipe or well, or the apparatus must be sheltered in a tide-house and kept heated; otherwise the record would be vitiated by the lack of data for certain months of each year. If the gauge is so poorly installed that it, or a pier or other structure with which it is connected, is subject to continuous variations of level, the record may become worthless for the purposes of precise determinations of mean sea level. Even where continuous variations of level are precluded, the record may become impaired because of occasional displacements effected during the setting of the instrument, because of a settling of the whole apparatus, or through some accident to the gauge or to the structure to which it is attached. For this reason, the reading scale of the gauge must be connected by precise levelling with permanent benchmarks on the solid land, so that the gauge may be reëstablished or the scale re-set in its proper position in case of injury. Reliable observers must faithfully care for the gauge and replace the recording paper at stated intervals, year after year, regardless of bad weather or other untoward conditions.

It is not surprising, in view of what has just been written, that few tidal stations provide proper data for the precise determination of mean sea level. So much care in the installation and operation of gauges costs money, and is not essential to the securing of such tidal data as ordinarily are desired by shipping and allied interests. For these interests the range of the tides, the approximate depth of water at wharves and in channels when the tide is low, the times of

high water and low water are significant; and to secure this information, reasonable care in installing and operating gauges is amply sufficient. An error of a few inches in the average depth of water in a channel is of small moment when a change in the wind direction may any day alter the depth by one or more feet. For the interests in question mean sea level has little practical significance, and its position is seldom determined. There are available in some ports abundant tidal data, secured during many years of continuous observation, which have never been utilized for calculating the mean level of the ocean's surface. Attempts to utilize such data for precise determinations of mean sea level have sometimes failed, because a gauge was installed on a pier which rose and fell with variations in load; because it was not connected with permanent benchmarks on the solid land by sufficiently accurate lines of level; because it had been displaced by an accident, and doubt existed as to whether it was replaced at precisely the same level as before; or for some other equally valid reason.

Government bureaus charged with the collection of tidal data, primarily for the use of shipping interests, have realized the importance of making their data so accurate that it would at the same time be available for other uses, such, for example, as determining the mean sea-level surface as a datum plane on which to base lines of geodetic levelling across the continent. In this respect, the U. S. Coast and Geodetic Survey, noted throughout the world for its high standards, takes an enviable rank. At a number of points along the coast it has maintained and operated gauges for long periods of years, using much care in selecting sites and effecting proper installations. High tribute must also be paid to the Canadian authorities, who have to cope with greater difficulties than do their neighbors to the south, both because of the exceptionally high range of tides on parts of their coast, and because of trouble from ice in their colder winters. The Canadian Tidal and Current Survey, now merged with the Canadian Hydrographic Office, has taken much pains to secure a degree of accuracy in its

work far beyond that required for the construction of the tide tables and charts published for use by shipping interests. This organization now has to its credit longer series of observations at several stations than have been made anywhere else except in New York Harbor and at a few tidal stations in India. How important are long series of records will be apparent from the following section.

III. Variations of Mean Sea Level Due to Meteorological and Astronomical Causes

In November, 1908, Dr. W. Bell Dawson, Superintendent of Tidal Surveys for the Canadian Government, read a paper on "Mean Sea Level at Quebec and New York" before the Canadian Society of Civil Engineers, in which he announced, as the result of precise tidal observations, that "there is a variation in Atlantic mean sea level from year to year, which is possibly periodic in a term of years." In 1917, the same authority published results of precise observations which showed that mean sea level as determined for one full year differed from the mean sea level of another full year by as much as .168 foot at Halifax, N. S., .172 foot at Charlottetown, P. E. I., and .307 foot at St. Paul Island in Cabot Strait. Dawson compared nine years of tidal observations at Halifax with observations at New York for the same years, and writes: "On comparing these values with the determinations at New York during the same periods of twelve months, making up these complete years, the variations correspond exactly. The lowest and highest values occurred in the same years at New York. . . . There can be no doubt, therefore, that there is an actual variation in the mean level of the ocean in different years. . . ." [1] Later, in a paper presented before the 1923 meeting of the American Geophysical Union at Washington, and entitled "Note on the Relative Accuracy of the Determination of Mean Sea Level and Extensive Lines of Land Levelling," Dawson stated that "there is undoubtedly an actual variation in the mean level of the sea from month to

[1] W. Bell Dawson, "Tide Levels and Datum Planes in Eastern Canada," 95 pages, Ottawa, 1917; see pp. 60, 68, 73, 74.

month and also from one year to another." In the opinion of this authority, the average value of three or four years might be considered as practically accurate, while an average value based upon five years or more would give a high degree of precision.

H. A. Marmer, of the U. S. Coast and Geodetic Survey, has pushed the analysis of variations in mean sea level still further. In papers [1] recently published, he has shown that precise tidal observations along the Atlantic coast extending over a number of years demonstrate great variability in the position of the mean sea-level surface. Marked changes in mean sea level occur from day to day, chiefly because of varying meteorological conditions. In February, when great changes in wind and weather are common, the greatest and least values of daily mean sea level may differ by nearly two and one half feet; in more equable June, by less than one foot.

Not only does mean sea level change from day to day, it changes also from month to month, and shows a periodic seasonal change in height. Long series of tide gauge observations made at different points along the Atlantic coast demonstrate that mean sea level is highest in the summer months and lowest in winter. At Portland, Maine, the seasonal variation of mean sea level in the course of a year amounted to three inches, while at Fernandina, Florida, it rises to one foot. These differences may be considerably increased by irregular, non-periodic variations of the monthly means due to variations in wind and weather. In other words, if an observer calculated the mean position of sea level at Fernandina on the basis of one month's most carefully made tidal records, and some years later made another month's careful observations at the same place, he might find a difference in mean sea level of considerably more than one foot; and he might attribute this difference to a rising or sinking of the coast when in reality it represented merely the normal fluctuations of mean sea level due to astronomical and meteorological causes.

[1] H. A. Marmer, "Sea Level along the Atlantic Coast of the United States and its Fluctuations," *Geographical Review*, Vol. XV., pp. 438–448, 1925. "Mean Sea Level and its Variations," *Assoc. Amer. Geographers, Annals*, Vol. XV., pp. 106–118, 1925.

Marmer's study further emphasizes the point brought out by Dawson, that even a whole year of the most accurate tidal observations is not sufficient to fix precisely the mean position of the ocean surface. Mean sea level changes from year to year, as well as from month to month and from day to day, although the annual fluctuations are still less pronounced than are the monthly variations described above. Nevertheless, mean sea level for one year may vary as much as three inches from mean sea level for the year following.

There exists, in addition, a fluctuation in mean sea level having a period of about four years, and another having a period of about nine years. From the middle curve of Fig. 4, it would appear that the average position of sea level at a given point during one complete four-year period, may differ from that of another four-year period by as much as two inches, even when no real change in the general elevation of land or sea is taking place. Similarly, the nine-year period (see lower curve, Fig. 4) may cause fictitious appearances of progressive emergence or submergence at the rate of nearly an inch in three years, or more than two feet per century. Thus, if we take the mean position of the ocean surface at Fort Hamilton, New York, for the nine-year period, 1893–1901, and compare it with the mean for the nine years 1896–1904, there appears to be a progressive subsidence of the coast or rising of sea level at the rate above stated. But within the next four years the (nine-year) average position of sea level falls noticeably, then rises again for a year or two, then falls more markedly for three or four years, until it is nearly back to the position with which we started. The maximum variations for the four-year and nine-year periods are even greater

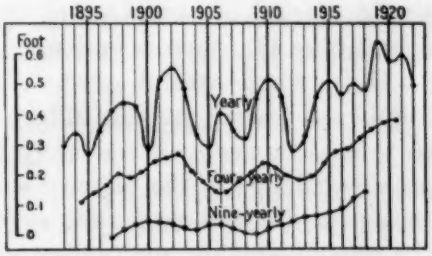

FIG. 4.—Yearly, four-yearly, and nine-yearly mean sea levels at Fort Hamilton, New York. (After Marmer)

than those indicated above; but since the curves show on the whole a gradual rise, especially during the last ten or twelve years, it may be argued that the maximum figures represent in part a real progressive submergence of the land. As our enquiry is now directed solely to local and temporary fluctuations of sea level believed to be wholly independent of any real change in the relative positions of land and sea, we must confine our attention to that measure of variation in mean sea level which appears to be clearly the product of meteorological, astronomical, or other extraneous forces. On this point, the evidence seems conclusive that the mean sea-level surface is so variable a feature that the average of nine years' accurate observations may differ from the average of another similar period of observations by an inch or possibly more, even where the coast is stable.

Only when careful observations have extended over a number of years,[1] and the differences in mean values amount to several inches, can we safely begin to suspect a real progressive change in the relative level of land and sea. Before the suspicion can be regarded as confirmed, we must eliminate the possibility that such larger changes of mean sea-level as are represented by the gradual rise (toward the right) of the four-yearly and nine-yearly curves of Fig. 4 are due to meteorological, astronomical, or other extraneous causes. For this purpose, we require additional observations in future years, which should make clear whether the rise in question is a constant feature, or merely part of a longer-period fluctuation, or the record of a gradual change in the tidal régime due to local modifications of the adjacent shores. This last point merits our special attention.

IV. Variations of Mean Sea Level Due to Variations in the Form of the Shore

It has frequently been assumed, even in technical circles, that once the variations of mean sea level due to meteoro-

[1] It is recognized that long-period observations are not necessary at a particular point, *providing* shorter-period observations there are comparable with longer period observations at some neighboring point on the coast.

logical and astronomical causes are eliminated, carefully made tidal observations afford a secure basis for establishing the datum plane required in geodetic levelling, and for determining possible changes in the level of the land. There seems to be every reason to agree with Dawson's statement that "Mean sea level . . . affords the only sure method of detecting . . . elevation or subsidence of the coast"; but we may hesitate to accept his further statement that "An average value based upon five years or more may be taken as correct in the absolute, as a basis for extended levelling. Any alteration of the value would indicate an elevation or subsidence of the coast region relatively to the ocean level." [1] Perhaps there are other causes of mean sea-level variation which operate more sporadically, but which are sufficient to negative the supposed absolute accuracy of long-period tidal observations.

The senior author has for many years believed that a potent cause of mean sea-level variations is to be found in the irregular and changing form of many shorelines bordering tidal seas. In other words, if precise tidal observations extending over a period of five years, or nine years, or nineteen years, give a certain value for mean sea level at a certain station; and a half century later another series of equally accurate observations for a similar period of time shows that the mean sea-level surface is three inches higher than formerly, he does not believe it safe to conclude that the coast has sunk three inches or general sea level risen three inches during the interval. He believes the difference may be local in extent, and the result of a change in the form of the shoreline. This theory of mean sea-level changes was presented to the Division of Geology and Geography of the National Research Council in 1922, and has since been briefly discussed before the American Philosophical Society and the National Academy of Sciences.

The junior author prepared, as a Master's thesis in physiography at Columbia University, a discussion of the causes of

[1] W. Bell Dawson, "Tide Levels and Datum Planes in Eastern Canada," 95 pages, Ottawa, 1917; see pp. 6 and 74.

local variations in mean sea level and a digest of the localities where physical conditions appeared to favor such local variations. The paragraphs which follow are based in part upon data collected in the course of that study.

Effect of Inflowing River Waters.—Let us consider first the simple and obvious case of a bay connecting with the sea by a narrow inlet and receiving the waters of inflowing rivers. It is well known that under such conditions the influx of river water will raise the mean level of the bay. Indeed, the difference in mean levels is often so distinct that the bay must be regarded rather as a lake, although at high tide the ocean level outside may rise well above the highest level ever reached by the water within the embayment. On the Pacific coast Vancouver Harbor is a bay connecting with the sea by a comparatively narrow inlet. The bay receives only a moderate amount of river water from several small streams. A tide gauge within the bay has been connected by precise levels with another outside the inlet, and from seven years' tidal observations at each station it has been found by the Canadian Hydrographic Office that mean sea level outside is 0.18 foot or approximately two inches, lower than the mean level within the embayment.

Lake Melville on the Labrador Coast, regarded by some as an arm of the sea constituting part of Hamilton Inlet, and an object of controversy in the recent Labrador Boundary Dispute, is connected eastward with the sea by a narrow channel through which salt water and the tides penetrate into the lake basin. From the west the basin receives the waters of Hamilton River as well as the contributions of smaller streams. Careful tidal observations made by the Canadian Hydrographic Office show that the mean level in the Lake is approximately half a foot higher than that in the open sea, although observations were not carried out long enough to give results of the highest precision. A still more striking example of this phenomenon is presented by Kennebecasis Lake or Bay, Fig. 10, which receives the waters of the St. John River and connects with the sea at St. John, New Brunswick,

by the very narrow tidal channel famous for producing the "reversible falls,"—from the sea into the embayment when the tide outside is high; from the embayment back into the ocean when the tide outside is low. According to the Canadian Hydrographic Office the mean level in the embayment is at least 2 feet higher than the mean level outside, since differences of 1.96 and 2.04 feet were determined by two lunar months' tide gauge readings during the autumn level of the river, the gauge in the embayment being well down toward the tidal inlet. A second period of observations in a later year gave a value for mean water level in the embayment 2.6 feet higher than the mean outside.

The foregoing examples are sufficient to show that significant differences may exist between the mean level of coastal embayments and that of the adjacent sea, in cases where the embayments impound or "lake" outflowing river waters. There can be no doubt that many such embayments along our coasts have abnormally high mean levels, the excessive elevation ordinarily amounting at most to but a few inches, but in some cases rising as high as a foot or more. What would happen if storm waves, tidal currents, or other agencies widened or deepened the inlets between sea and embayment, or created additional inlets, so that better egress of waters from the embayment to the sea would be insured? Obviously the ponding of the river waters would be less effective, and the mean level of the embayment would fall. Thus would be created, within the embayment, fictitious indications of an uplift of the land. Or suppose that the inlet were gradually narrowed or shallowed through the deposition of débris by wave or tidal currents, so that the escape of the river waters was more and more obstructed. In this case the mean level within the embayment would gradually rise, and one would find there fictitious indications of a gradual subsidence of the coast. Were the most careful observer to carry on for a period of years highly accurate tidal measurements in a bay or harbor of the type here considered; and were he or another observer to make similar precise measure-

ments, say half a century later, with the result of discovering a difference of several inches between the mean levels observed at the two periods, a conclusion in favor of coastal subsidence might follow, when in reality the growth of a sandspit or other shore changes were actually responsible for all the observed facts. Such shore changes are of constant occurrence, and we believe they frequently cause significant variations in mean sea level. Bays or harbors with wide mouths will be least affected; but this disturbing force must be recognized as a possibility wherever outflowing river waters have even a slight effect in raising the mean level of an arm of the sea.

Effect of Wind Direction.—Impounded river waters are not necessary, however, to the production of local variations of mean sea level consequent upon changes in the form of the shoreline. Tidal waters alone are subject to similar fluctuations of mean level. Let us consider first the case of mean sea level as affected by prevailing wind directions. The power of the winds to disturb water levels is well known to those living along the shores of seas or large lakes. In Pamlico and Albemarle Sounds some shallow areas are laid dry when the winds blow strongly in a given direction, while shallows to leeward have the water depths over them temporarily increased. Boats running from Titusville, Florida, across the lagoon to Cape Canaveral are unable to make the trip when strong winds from a certain direction blow much of the water from the area to be traversed and pile it up against lee shores. In the Baltic strong north winds lower sea level in the Gulf of Bothnia, at the same time raising the southern part of the sea 6 feet or more until the streets of Trelleborg and other towns of south Sweden are inundated. Sea level at Galveston, Texas, was raised more than 12 feet by the hurricane of September, 1900.

It is not difficult to understand that if the wind blows constantly in a given direction, the level of a water body over which it blows must be permanently distorted, with an abnormally low level toward the windward shore, and an abnormally high level toward the lee shore. Or if winds are

variable, but have a prevailing or dominant set in a given direction, then the average or mean level of the water body will be similarly distorted. A landmass separating two water bodies so affected, will have distinctly different mean water levels on its two sides, as shown at L in Fig. 5. Precise levelling across the Isthmus of Panama and tidal observations on the two sides show that mean sea level at the Pacific entrance to the Panama Canal is about 8½ inches higher than mean sea level at the Atlantic entrance, while precise levelling across the United States indicates that in this latitude Pacific mean sea level is about 2 feet higher than Atlantic mean sea level.[1] The latter line of levels is too long to give a comparison of great precision, but there seems no reason to doubt the existence of a higher sea level in the Pacific. At both localities mentioned the difference in levels is attributed to winds and other weather conditions.

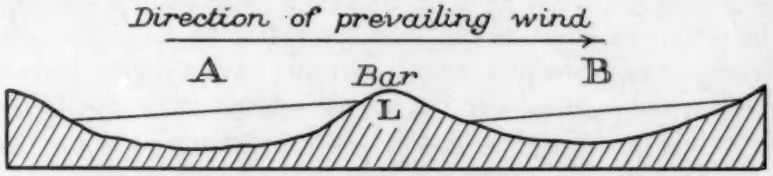

FIG. 5.

Let us now imagine that in Fig. 5, one of the water bodies (*A*) represents the ocean, while the other (*B*) represents a bay or lagoon separated from the ocean by a bar (*L*) through which a narrow tidal inlet connects the two water bodies. Through this inlet the tides will ebb and flow, but so long as the inlet is narrow a difference in mean sea level on the two sides of the bar (*L*) will persist. If now the inlet be greatly widened, or if additional inlets be cut through the bar, by storm waves or otherwise, so that the waters of *A* flow freely into *B*, mean sea level on the two sides of the bar will approach equality. This will probably involve a lowering of mean sea level on the side of the bar toward the ocean (*A*), and a rise of

[1] Personal communication from Mr. H. A. Marmer, U. S. Coast and Geodetic Survey.

mean sea level on that side of the bar facing the bay or lagoon (*B*). A comparison of tidal observations made on the seaward side of the bar before the shoreline changes described, with similar observations made afterward, would give a fictitious indication of coastal elevation. Similar studies made on the lagoon side of the bar, before and after the changes in the inlets, would give a fictitious appearance of coastal subsidence. The extent of local changes in mean sea level which may arise from this cause will vary greatly according to the average strength of the winds, degree of dominance of winds from a single direction, shape of containing basins, size of inlets, and other factors; but it seems probable that under favorable conditions it may amount to a number of inches.

Effect of the Earth's Rotation.—Tidal conditions alone, unaffected by either river inflow or wind direction, will produce local inequalities of mean sea level which will change with changes in the form of the shore. Let us first consider the deflective effect of the earth's rotation on tidal currents. Marmer [1] has shown that tidal currents have a higher surface level on the right side, in the northern hemisphere, due to the deflection of the waters by the rotating earth. Thus in New York Harbor tidal ranges are found to be higher on right-hand shores than on left-hand shores. Where the currents ebb and flow through the same channel, mean sea level may be approximately the same on both sides, although compensation may not be wholly perfect, due to a predominance of one current over the other. It is well known, however, that in some cases flood tide follows one channel, ebb another, as where islands divide the channels, or other irregularities introduce peculiarities into the tidal movements. Under these conditions it may happen that along a given shore mean sea level is abnormally high or abnormally low. If later changes in shores or channels deflect the tidal currents into new courses, a readjustment of mean sea level will result, giving fictitious indications of coastal elevation or coastal subsidence. Theoretical considerations lead to the conclusion that in

[1] Marmer, H. A., "Tides and Currents in New York Harbor," *U. S. Coast and Geodetic Survey*, Special Pub. No. 111, 174 pages; p. 56, 1925.

extreme cases the change in mean level might be measurable in inches.

Effect of Nature of Tidal Inlets.—Imagine a tidal bay or lagoon, Fig. 6, connected with the ocean by an inlet across the

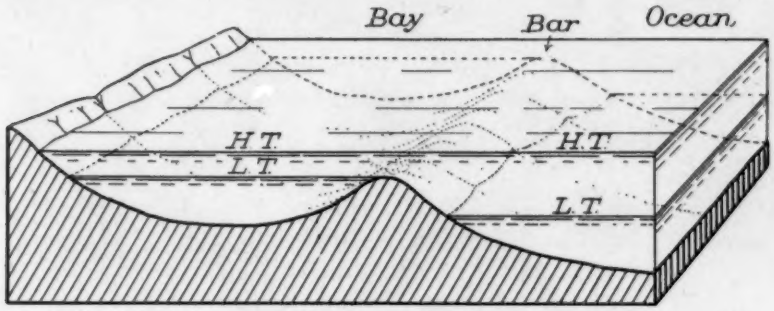

FIG. 6.

intervening bar so shallow that at low tide the waters of the bay cannot wholly escape. Under these conditions high tide in the bay may be the same as that in the open ocean, if the inlet, notwithstanding its shallowness, is broad enough to admit the high tide freely. But low tide in the bay, as is shown by the figure, can only fall to the level of the shallow inlet across the bar. It is obvious that mean sea level in the bay must, under the assumed conditions, be higher than mean sea level in the ocean.

Let us now imagine that the bar of Fig. 6 is trenched by a narrow slit or channel, deep enough to permit all the water to drain out of the bay were time allowed for such drainage, but so narrow that long before such drainage can occur the tide in the ocean rises and begins to pour back into the bay. Such are the conditions represented in Fig. 7. Again high tide in the bay will rise to the level of high tide in the open ocean. Low tide in the bay will fall lower than formerly, because of partial outflow through the narrow passageway now cut in the bar; but since the channel is too restricted for free outflow, low tide in the bay never falls as low as low tide in the ocean, the rising waters of which during the next high tide stop the draining process and reverse the flow. Obviously mean sea

level in the bay is lower than under the conditions represented in Fig. 6; but it is still distinctly higher than mean sea level in the open ocean.

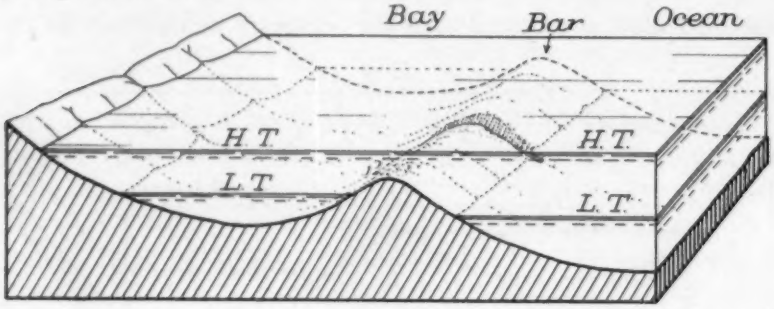

Fig. 7.

Let us now vary the conditions by assuming that the main inlet, instead of having slight depth and indefinite breadth as provided in the two cases considered above, shall be deep but so narrow that when the tide is high in the ocean the waters flowing into the bay are unable to fill it before the ocean level begins to fall. Similarly, when the tide is low in the ocean the waters of the bay cannot escape fast enough completely to reduce the bay level before the ocean level begins to rise again with the next high tide. Then, as shown in Fig. 8, high tide

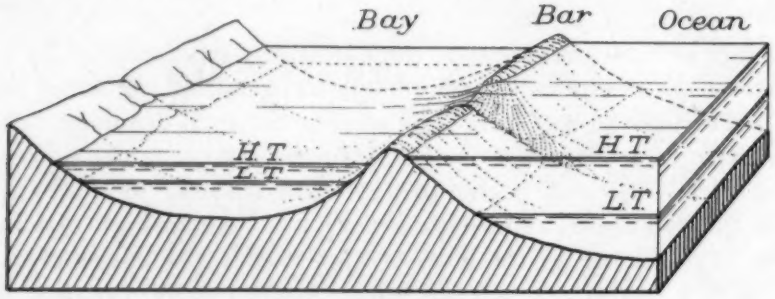

Fig. 8.

in the bay will always be lower than that in the ocean; while low tide in the bay will always be higher than the ocean low tide. If we assume that the inlet is wider at the top than at

the bottom, permitting much freer passage to the waters when the tides are high than when they are low, the high tide in the bay will not differ from high tide in the ocean by so great an amount as will the two low tides differ (see Fig. 8). Under these conditions again mean sea level in the bay may be significantly higher than mean sea level in the ocean.

Certainly the conditions represented in Fig. 8 are encountered more frequently in nature than are those represented by Figs. 6 and 7. Yet all are possible, and it is now to be remarked that all are peculiarly susceptible to disturbance consequent upon changes in the form of bars and inlets. The shallowing or deepening of inlets; the growth of bars across the mouths of bays formerly free from such shore features, or the destruction of bars by storm waves; the narrowing of inlets by sand-spit growth, their widening by wave or current action, or the breaching of bars by new inlets formed by storm waves or by the outburst of impounded land waters; any and all of these must be potent causes of local changes in mean sea level in harbors, bays or lagoons where are found conditions approximating those described above. Nor do the conditions described exhaust the list of those which may give rise to local differences in mean sea level. They are merely examples intended to illustrate the fundamental principle that local changes in the form of the shore may, under appropriate conditions, produce local changes in mean sea level. Such changes in mean sea level may be gradual or sudden, depending on the nature of the shore changes responsible for them; and they may amount to fractions of an inch or to a number of inches, depending on the form and size of inlets and bays and on the range of the tides. Where gradual and imperceptible, yet of significant amount, they are apt to be wrongly attributed to a progressive, slow coastal elevation or coastal subsidence.

There is reason to believe that some of the discrepancies in the position of mean sea level as determined at different points, heretofore tentatively ascribed to errors in levelling or to errors in tidal observations, are real differences due to the

disturbing effect of shore form upon mean sea-level elevation. Hence the practice of distributing the "error" along a line of precise level at the two ends of which mean sea level shows divergent values, is likely to introduce errors which did not previously exist. A pertinent commentary, on the effect of mean sea-level differences in throwing suspicion on tidal observations which may prove to be quite correct, is contained in the following statement kindly furnished by Mr. F. Anderson, the Chief Hydrographer of Canada: "That there are areas in the Gulf of St. Lawrence where the local mean sea level is not the same as the true mean sea level seemed apparent some years back, when the elevation of mean sea level as carried by the line of precise levels from Halifax to Father Point did not check with the local determinations from short periods of records at Port Daniel in Chaleur Bay, and Fox River on the Gaspé Peninsula. At Port Daniel from two lunar month's reduction in each of the years 1913 and 1917 the local mean sea level was found to be .75 ft. lower, and at Fox River from four month's record in 1918 it appeared to be .77 ft. lower, while at Father Point the level checked with the mean sea level value from ten years' record by .02 ft. The discrepancy is so large that we were inclined to believe that there was something wrong somewhere. . . . It was because of the unreconcilable levels alone that the gauge was put at Port Daniel for the second year, in order to determine if by any chance there had been an error of levels. Again, at Cape Tormentine a tabulation of five months of tidal record gives a mean sea-level value of .10 ft. below mean sea level as brought from Halifax, while a few miles away in Bay Verte, as determined from two months of record at Tidnish and Port Elgin the local mean sea level appears to be half a foot higher."

After the preceding discussion had been written the writers learned that Mr. Chester K. Wentworth had independently given consideration to the difference which must exist between mean sea level in a nearly enclosed lagoon and in the open sea, where the two water bodies are connected by narrow inlets which have greater breadth at high tide than at

low tide. As a result of theoretical computations based in a general way on conditions observed in the lagoon of the Fanning Island atoll in the North Pacific, Wentworth concluded that "with a tidal range of three or four feet the superior elevation of the inner mean tide over the outer mean tide may amount to as much as a half foot or more, under no very extreme assumptions as to shape of channel and size of lagoon." [1] From data generously placed at our disposal it would appear that Fanning Island and other members of the Line Islands group afford special advantages for studying differences in the position of mean sea level caused by the form of tidal inlets and the direction of prevailing winds, in the absence of any disturbing effects from inflowing river waters.

V. Need of Further Tidal Studies

It was theoretical considerations like those set forth above that led the senior author several years ago to suggest to the Division of Geology and Geography of the National Research Council the desirability of prosecuting special tidal studies designed to increase our knowledge of the nature and form of the mean sea-level surface. This suggestion led to the formation of a Committee on Shoreline Investigations operating under the auspices of the Division. A program of tidal studies elaborated by the senior author and involving systematic observations at selected points on the Atlantic Coast, was approved by the Committee and effectively supported by officers of the Division. Work was begun in the vicinity of New York Harbor, where studies are now in progress under the general direction of the Committee. Five large automatic tide gauges, of the standard type employed by the Coast and Geodetic Survey in its most accurate work, were installed by officers of the Survey, three at selected points in Jamaica Bay, one at Fort Hamilton in the entrance to Upper New York Bay, and one at the Battery on the southern end of Manhattan Island. The installation, housing and operation of these gauges was made possible

[1] Chester K. Wentworth, *personal communication.*

through the generous coöperation of the United States Coast and Geodetic Survey, the Department of Docks of New York City, and the Department of Plant and Structures of New York City. The directors and officers of the three organizations have afforded every assistance within their powers, and have thus made possible an investigation the expense and labor of which would otherwise have proven prohibitive. Valuable assistance has likewise been rendered by Prof. J. K. Finch of the Department of Civil Engineering of Columbia

FIG. 9.—Tide gauge stations (black triangles) in New York waters utilized in tidal studies described in text.

University, who has from the first placed his advice and his engineering skill at the service of the Committee.

The New York region was selected because a variety of conditions combined to make it especially appropriate as a field of study. Jamaica Bay, Fig. 9, offers a good example of a moderate-sized embayment separated from the ocean by a sand bar through which an inlet of considerable depth and moderate breadth permits tidal waters to enter and leave the bay. Very little river water enters the bay, the tidal range both in the ocean (4 to 6 feet) and in the bay is moderate, and

winds are variable. Under these conditions the expectable differences in mean sea level must be very small, at most a very few inches and possibly only fractions of an inch. If any differences occur here, similar and larger differences must be of frequent occurrence along our own and other coasts. In this respect the New York region is a test case of much value. To determine such small differences of level with accuracy the gauges must be connected by lines of precise level made with a high degree of refinement; and the New York region is covered by a precise level net of this type, surveyed for the New York City Board of Estimate and Apportionment in 1910–12 by Frederick W. Koop, who took a keen interest in the problem of coastal subsidence and discussed it at some length in his report on the precise level operations.[1] The three gauges in Jamaica Bay are so placed as to form a triangle, tidal observations at the apices of which must reveal any appreciable distortion of the mean sea-level surface in the bay due to predominance of winds from a given direction or to other causes. Extensive dredging and filling operations are in progress in the bay, under the direction of U. S. Army Engineers. Opportunity will thus be afforded to detect any changes in the tidal régime and any fluctuations of mean sea level due to artificial changes in shore outline and in channel breadth and depth. For this latter purpose observations after the lapse of some years and after the extensive program of dredging and filling is well advanced, will probably be necessary.

It should fully be understood that the tidal studies in progress and projected for the future are of a two-fold nature. On the one hand they contemplate intensive studies over a comparatively short period of time, say one year at each locality, for the purpose of determining the *form* of the mean sea-level surface as affected by the form of the shore combined with the influence of outflowing rivers, prevailing winds, and other factors. On the other hand, they involve comparative

[1] Frederick W. Koop, "Precise Leveling in New York City," City of New York Board of Estimate and Apportionment, 263 pages, New York, 1914.

studies at the most favorable points 25, 50, or 100 years hence, for the purpose of determining whether or not there has been within the interval any such change in mean sea level as would indicate an elevation or subsidence of the coast. Studies of the first type have not heretofore been made. Such studies of the second type as have been made are usually of little value, because studies of the first type, upon which the validity of later observations must depend, were not prosecuted. The truth of this will appear more clearly in a later paragraph.

FIG. 10.—Suggested tide gauge stations (black triangles) for proposed tidal studies in New Brunswick waters.

In striking contrast with the New York region is the St. John district, New Brunswick. Here the Kennebecasis embayment, Fig. 10, is of large extent, the influx of river water is great, and the connection with the sea is by means of an extremely narrow channel of limited depth. The tidal range in the sea is great, 20 to 25 feet, while that in the embayment is extremely small, 1½ or 2 feet. Such tidal observations as have been made indicate that the mean level of the embayment is at least 2 feet higher than that of St. John Harbor outside. Conditions are here peculiarly favorable for a study of the extent to which the varying amounts of water discharged into an embayment by a large river may affect the elevation of mean sea level. If possible, the part of such elevation due to river discharge should be differentiated from that due to tidal waters operating through a narrow channel of given form, to prevailing winds, and perhaps, to other factors.

The barrier between the embayment and the sea in the St. John district is composed of rock and glacial debris, instead of a sand bar deposited by marine agencies as is the case at Jamaica Bay. This, however, is immaterial to the present study, which deals with possible distortions of sea-level surface in all kinds of embayments, regardless of whether the distortions in any particular area will vary rapidly with rapid

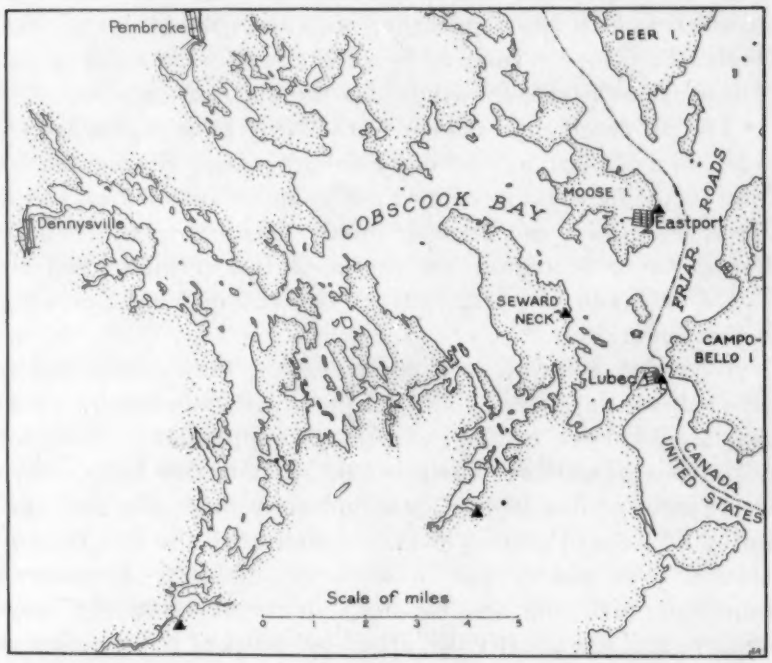

FIG. 11.—Suggested tide gauge stations (black triangles) for proposed tidal studies in Maine waters.

changes in the form of unstable shores, or will vary imperceptibly with slow changes of a more resistant coast.

The vicinity of Eastport, Maine, Fig. 11, is a third region important for tidal studies of the type here described. In many respects Eastport is intermediate in regard to its tidal conditions, as it is in point of geographical position, between the New York and the St. John areas. Large embayments

are connected with the sea by inlets of fair size, the influx of river water is of moderate amount, and the tidal range (18 or 19 feet) is greater than at New York but less than at St. John. Special interest attaches to tidal conditions here because of the project for developing tidal power on a large scale which received the sanction of the voters of Maine at a recent election. The Committee on Shoreline Investigations has received a promise of substantial coöperation from the engineering firm most concerned with the tidal power project, Dexter P. Cooper, Inc., to become effective as soon as tide gauges are available for studies in the Eastport region.

The proposed tidal studies in the New York region are not only assured, but are actually in progress. Studies at St. John are being considered by the Canadian authorities, but at this writing have not been definitely decided upon. Studies in the Eastport region are provided for, if and when the U. S. Coast and Geodetic Survey can extend its coöperation to that area.

Whatever the results of these studies, they cannot fail to have substantial value. Thus, if the investigations now under way in New York waters show that no appreciable distortion of the mean sea-level surface can be detected by a year's observation of five gauges operated simultaneously and connected by lines of precise level, we shall have the first demonstration ever made that in bays of moderate dimensions connected with the sea by deep inlets of relatively large breadth, and not greatly disturbed by influx of river waters or prevailing winds, mean sea level is an approximately level plane of constant value throughout the region, and hence not apt to be appreciably affected by ordinary changes in the surrounding shores. If, on the other hand, the New York studies indicate that even where bays and inlets of the type just described are concerned, mean sea level is by no means "level," but shows appreciable differences of elevation at different points, then we shall have the first substantial proof that the mean sea-level surface is so sensitive to irregularities in shore form that it is likely to alter its position with normal

changes in shore outline. As corollaries we could then draw the further conclusions that harbors and bays are unfavorable locations for tide gauges intended to give accurate and constant values for mean sea level as a datum plane on which to base geodetic surveys of high precision; that serious distortions of mean sea level must exist at many places on our own and other coasts, where conditions are much more favorable to such distortions than in the New York region; and that apparent small changes in the position of mean sea level with respect to the adjacent land should not be accepted as indications of coastal elevation or coastal subsidence, since they may well represent merely local fluctuations in the position of mean sea level consequent upon local changes in the character of neighboring shores.

It should further be observed that whatever results are obtained by the present tidal studies, they will form the best basis yet secured for testing the theory of a progressive subsidence of the Atlantic Coast. After the lapse of 50 or 100 years, tide gauges reëstablished in the same localities and operated for a year, would serve to confirm the essentially stable form of the mean sea-level surface in the affected area, or to detect any local changes in that surface due to changes in the form of the shores. By comparing the results thus obtained with the record of some one gauge (like that at Fort Hamilton) operated for a period of years sufficiently long to eliminate irregular and periodic fluctuations of mean sea level due to meteorological and astronomical causes, a solid basis would be laid for determining whether or not there had been any general progressive change in the relative levels of land and sea. To-day we are unable to make such a determination for the Atlantic Coast, because the supposed changes are slow as to rate and small as to amount, and our predecessors failed to make observations of sufficient precision to serve as a reliable basis of comparison. We at least shall hand on to our successors data of far greater precision than any at our disposal, and which, so far as we can determine in the light of our present knowledge, will be adequate for the purpose in view.

The writers desire to acknowledge special indebtedness to G. T. Rude and H. A. Marmer of the U. S. Coast and Geodetic Survey for helpful data and criticisms, and to thank the many others, in number too large for individual mention, who have placed needed information at our disposal.

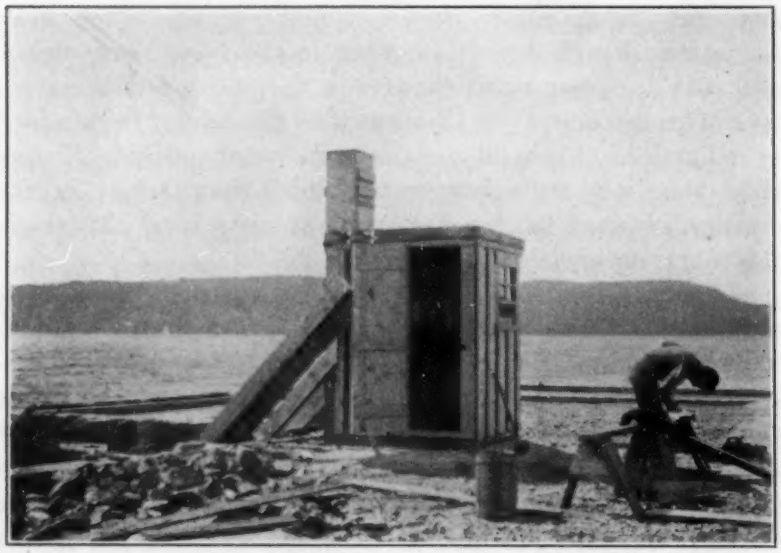

FIG. 12.—A tide gauge installation in Canadian waters. Pipes for floats go down through ballast-floor of wharf. There is a square wooden casing (3 ft.) to ballast-floor, and electric heaters, reflecting type, playing on pipes above water line to keep the water from freezing in and around pipes. The high chimney-like box in rear is for a temporary gauge during freshet, when the waters cover the wharf. (Photo by H. W. Jones of the Canadian Tidal and Current Survey.)

METABOLIC CHANGES IN THE BODY OF FEMALE PIGEONS AT OVULATION

By OSCAR RIDDLE

(*Read April 30, 1927*)

Earlier work has shown that when female doves and pigeons are induced to ovulate repeatedly in rapid succession from winter to late summer an effect becomes notable in their ova, or germ cells.[1] The ova produced under these conditions normally respond by the storage of additional material within themselves; the "yolks" become heavier. This change in the germ cell represents a reduction in its oxidizing power, *i.e.*, its metabolic level, and has been shown to be of importance to our knowledge of the physiological nature and basis of sex. The question at once arises, do notable and predictable metabolic changes occur also in the soma, or body, at each ovulation period? Or, are somatic metabolic changes limited to seasonal changes? A special consideration of the first of these questions is the purpose of this communication.

It is not necessarily to be expected that the metabolism of the body as a whole will run parallel with that of the ova, nor that metabolic changes in the body are the *cause* of the changes within the ova; for, we know of instances in which the germ plasm—the ova—appears to act counter to the body tissues, or somatoplasm. However, in any particular species the metabolic changes in the ova may be expected to follow one or another rule—not several different rules—with respect to fluctuations in the metabolism of the body or soma. As an example of a divergence of the metabolism of somatic and germinal tissues we may cite the well-known case of the salmon, in which the ova develop (store rapidly, *i.e.*, *low*

[1] Riddle, O., *Amer. Nat.*, Vol. L., p. 385 (1916) (also earlier and later papers).

metabolism) at the expense of the body and muscles at a time of great muscular activity (*high* somatic metabolism) incident to the long migration up-stream to the breeding grounds. Another example has been provided by some experiments of our own on a bird. We were able to show that under certain, not all, conditions a laying fowl can be entirely deprived of food without preventing the continued growth and ovulation of some of her germ cells; in one instance four eggs thus matured and the last of these was laid on the twelfth day after the withdrawal of food. In these cases it is clear that despite the low burning power of these ova or yolks they succeeded in robbing all the other tissues of the body of the very materials upon which the life of the body depends. Other examples could be cited. We therefore approach the present topic—the question of somatic metabolic change at ovulation without a basis of prediction as to the nature of the changes which may be found.

A digression here will make it easier for the reader to bring the present results into a proper relation to our main studies on sex. In those studies it was found that not single ovulation periods, but repeated and continuous ovulation periods, precede the increased storage in the egg and lead to the observed modifications of sexuality, fertility and longevity in the offspring. Such repeated and continuous ovulations extend themselves over many or most months of the year. In pigeons the laying period may be thought of as beginning in late winter, as continuing through spring and summer, and usually as being terminated or much reduced in the early autumn. The effects of the individual ovulations appear to be cumulative, and are more pronounced by the end of summer; they are least evident or usually indeed not at all present at the beginning, *i.e.*, near the end of winter. It thus results that the effects of continuous ovulation are in some way—directly or indirectly—also associated with season.

On the basis of season some somatic changes of much interest to the present discussion have been observed. The body-weight of the female (and male) pigeon is slightly re-

duced from a maximum winter to a minimum late summer value. The thyroid glands undergo a marked reduction during this same period. The ovaries increase in size during this period—particularly during the spring. The blood calcium (parathyroid activity) probably increases from winter to summer. These several somatic seasonal changes have been observed and reported elsewhere. Such observations make more evident the importance attaching to a knowledge of the somatic metabolic changes which accompany each single ovulation period, since a summation of the effects of single periods seems to be primarily involved in the results—increased storage in the ova, and increased femaleness derivable from such ova.

Some parts of the present investigation have been shared with various collaborators. Though some aspects of the study have been published, entirely new phases of the problem have now been investigated and the general problem has hitherto not been considered. Studies have been made on: (*a*) size changes in various organs, particularly in the endocrine organs; (*b*) blood changes; (*c*) other functional or metabolic changes.

Most of the data shown here have been obtained on ring doves and confirmed in common pigeons and in hybrids. The results can be best presented in the form of point-to-point curves (Fig. 1). Though a scale of values can not be provided for each of the several quantities expressed on the curve, a common base-line serves for them all and the extent of fluctuation of each value is shown in its true proportion.

The Final Growth Period of the Ovum

We early found that at very close to 108 hours before an ovum is liberated from the ovary (ovulation) this ovum quite suddenly begins a much faster rate of growth than obtained in it during several preceding weeks. This change in growth rate is marked also by a change in the color and nature of the yolk formed. In the fowl we found that this final growth rate was about 26 times the earlier rate.[1] Evidence has been

[1] Riddle, O., *Amer. Jour. Physiol.*, Vol. XLI., p. 387 (1916).

obtained for a quite equivalent increase in the growth rate in the ovum of the pigeon; and the increase there occurs when the ovum has attained a diameter of 4–5 mm. The curve

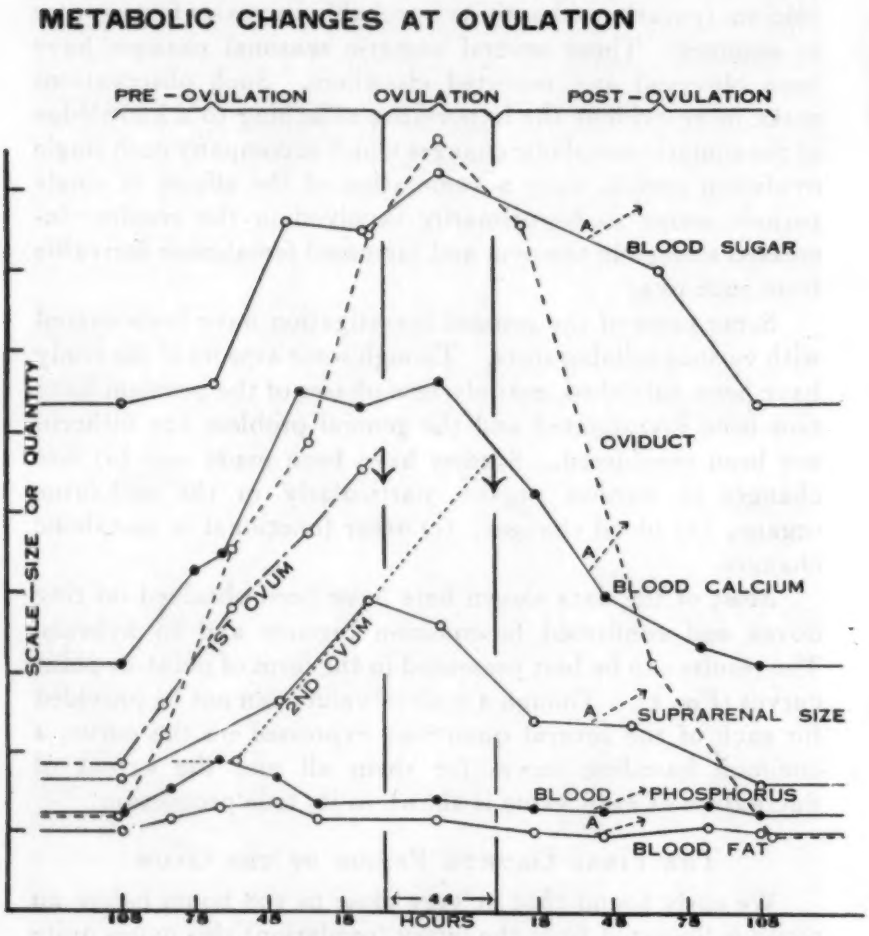

FIG. 1—For explanation see text.

showing the growth of the "first" ovum is based on measurements of the diameter of its longest axis, and an increase from 4.6 mm. in the resting ovum to 14.7 mm. at 8 hours before ovulation was found. The curve for the "second" ovum is

diagrammatic except for measurement at the point of change to an increased rate of growth. These two ova separate from the ovary, *i.e.*, ovulate, at fixed hours in pigeons. These hours differ slightly in common pigeons and in ring doves. The mean interval between these two ovulations in pigeons and doves is about 44 hours. This interval has been called by us the "ovulation" or "mid-ovulation" period. The 108 hours included in the final growth period of the "first" ovum is called the "pre-ovulation" period. It is normally preceded by days or weeks of a "resting" period during which the ovary is in a resting or unchanging condition. The 108 hours following the dehiscence of the "second" ovum from the ovary is called the "post-ovulation" period. Its duration was first fixed by the observation that the oviduct required this length of time to return to its normal or resting weight in birds not permitted to incubate following egg-laying. The additional values for blood calcium, sugar, etc., now plotted in the form of curves on Fig. 1 also almost uniformly require a period of approximately 108 hours to return to the normal or resting value.

If we knew what it is that initiates this final period of rapid growth in the ovum we would probably have the key to several or most of the changes shown on Fig. 1. The ovary itself is probably directly concerned in the increase of blood fat and of lipoid phosphorus. The hormone produced by the ovary, or by the growing follicle, almost certainly provides the stimulus for the extraordinary growth of the oviduct. Thus these four changing values are, in all probability, traceable to the ovary and their simultaneous occurrence is therefore readily understood. It seems not improbable that a close functional relationship between the ovary on the one hand and the parathyroids and suprarenals on the other is also responsible for the size changes in the latter and for the observed changes in blood sugar and blood calcium. Concerning the directive agent for ovarian action we may refer to very recent results which identify the anterior pituitary body with this function. Smith, and Zondek and Aschheim have obtained

evidence that a hormone secreted by the mature pituitary quickly initiates the development of ova in immature ovaries of rats and mice. Obviously the metabolic changes within the growing ovum are *germinal* changes and therefore fall outside the somatic or body changes under discussion here.

Size Changes in the Oviduct

The ring dove oviduct in resting condition weighs approximately 0.6 gram, while during ovulation it averages 5.1 gms. In common pigeons these weights are 1.0 and 11.0 gms., respectively. Thus in ring doves the increase during a period of about 120 hours is 800 per cent.; in common pigeons it is 1100 per cent. This organ consists of three different glands which secrete the three distinct egg-envelopes—albumen, shell-membrane, and shell—of the pigeon's egg. It is known that the size increase of the large albumen and shell-secreting parts of this organ is not wholly a growth of cellular structures, but is partly an increase in vascularity, and partly a *storage* of substance soon to be excreted as albumen and calcium carbonate. It was pointed out above that this extraordinary development of the oviduct is initiated by a hormone produced in the ovary, possibly in an ovarian ovum that is beginning its period of rapid growth. It is very difficult to think of the ovary as a whole being the principal contributor of the hormone in pigeons and other birds since, as perfectly shown in Fig. 1, the oviduct immediately involutes when an ovum in the final period of rapid growth is no longer present in the ovary.

What the rapid and enormous growth of this glandular organ means in terms of metabolism is uncertain. One has much reason to believe that the period of greatest secretory activity, certainly the most active excretion, falls within the early period of oviducal involution rather than in that of growth. It is certain only that at each ovulation period this organ undergoes phenomenal growth. The relation of the anterior pituitary both to general bodily growth, and to the initiation of the growth of an ovum—an event with which the

production of hormone for oviducal growth is somehow associated,—leads us to interpret our data as suggesting both a direct and indirect action of the pituitary hormone in this cyclical growth of the oviduct of the pigeon.

Suprarenal Hypertrophy at Ovulation

Both medullary and cortical portions of the suprarenal have been found hypertrophic at ovulation.[1] The mean weight increase for the entire gland in four different groups or species of pigeons was nearly 40 per cent. above the normal. The hypertrophy of the two portions of the gland suggests an increase in their action or function. According to the studies of Marine an increased functioning of the suprarenal cortex should tend to oppose the action of the thyroid. If this is true a continuous hyperfunction of the cortex, corresponding to nearly continuous hypertrophy under forced or rapid egg-laying, should tend to reduce the metabolic rate in the somatic tissues. If this action extends also to the germinal tissues it may play a part in the reduced metabolism that has been demonstrated to occur in the ova. It is certain, as noted above, that the season of highest storage in the ovum, and of greatest predominance of femaleness in the offspring, is the season of smallest thyroid size.

An increased action of the suprarenal medulla, however, means an increased output of adrenin and this may be expected to raise the metabolism of the somatic tissues. In order to learn whether an increased activity of the medulla actually occurs at ovulation, a study of the blood sugar curve was undertaken. The results—described immediately below—suggest that such an increase of activity does in fact occur coincident with the suprarenal enlargement and with ovulation. From this study it is fairly evident that the metabolism of the somatic tissues is notably affected by the enlargement of each of the two parts of the suprarenal; but the effects of increased action of the two parts appear to be divergent, and the actual effect on the somatic metabolism

[1] Riddle, O., *Amer. Jour. Physiol.*, Vol. LVI., p. 322 (1923).

cannot now be estimated. Earlier workers have almost uniformly reported an increase of suprarenal size in frogs, birds and mammals at the breeding season.

Blood Sugar

Each ovulation period in pigeons is accompanied by an increase of approximately 20 per cent. in the amount of sugar in the blood.[1] Female pigeons forced to rapid and almost continuous ovulations throughout the year are thus forced to an almost continuous maintenance of their bodily functions—and to the production of their eggs—under altered and unusual conditions of carbohydrate metabolism. As noted above, an increase in blood sugar is an expected result of an increased secretion of adrenin. And adrenin increases metabolism. It is also true, however, that the level of glucose in the blood is much influenced by quite other factors than adrenin. It may perhaps be questioned whether the increased sugar values represent increased mobilization or decreased utilization of the carbohydrate stores. This question receives emphasis from the fact that so much of the endocrine system is already seen to be functioning at an unusual level at ovulation (Fig. 1). Some importance may attach to the fact that ovulation in pigeons may be easily suppressed or greatly restricted by injections of insulin. This and some other (rather inadequate) observations have suggested that a pigeon's capacity to ovulate is completely blocked if the blood sugar is held at or below normal.

Parathyroids and Blood Calcium

The parathyroid glands are now considered of chief importance in the mobilization of calcium and in the maintenance of the calcium level in the blood. In studies of the blood calcium therefore we seem to have an opportunity to measure fluctuations in the activity of the parathyroids—the higher calcium values reflecting the more active functioning of the glands. It was found that the "resting" or normal calcium value in the blood of pigeons is raised during ovulation

[1] Riddle, O., and Honeywell, H. E., *Amer. Jour. Physiol.*, Vol. LVI., p. 340 (1923)

from 9.3 to 19.9 mgms. per 100 cc.[1] It is more than doubled. A similar change during egg-laying has recently been described in fowls.

This profound change in the calcium metabolism is not known to involve any change in basal heat production. It is conceivable, however, that it may notably affect the permeability of the membrane (follicular) which surrounds the growing ovum. This membrane apparently becomes much more permeable at the time an ovum begins its final growth period—and this period is coincident with the beginning of increased calcium concentration in the blood. It is also to be noted that the normal calcium level of the blood of pigeons seems to increase from winter to summer; and that Grant and Gates have observed in rabbits an increase of parathyroid size from winter to summer.

The increase and decrease in blood calcium at ovulation are quite coterminous with the above described increase and decrease of size in the oviduct and suprarenal; with increase and decrease of the blood sugar; and with the increase in size of the particular ovum which will ovulate 108 hours later. The time of maximum increase—at ovulation—is apparently the same in all. The seasonal maximum of calcium seems coterminous with largest size of testis and ovary; with smallest thyroid size in the parent; and with the greatest excess of females in the offspring.

Blood Fat and Lipoid Phosphorus

During the ovulation cycle of female ring-doves the amount of alcohol-ether-soluble substance in the whole blood is increased to 35 per cent. above the normal or "resting" value. The phosphorus contained in this extract is increased to approximately 50 per cent. above the normal value.[2] The point of chief interest here lies in the fact that the highest values for blood fat and phosphorus do not coincide with the

[1] Riddle, O., and Reinhart, W. H., *Amer. Jour. Physiol.*, Vol. LXXVI., p. 660 (1926).

[2] Riddle, O., and Burns, F. H., *Amer. Jour. Physiol.* Vol. LXXXI., p. 711 (1927).

middle of the ovulation period but precede it by 67–45 hours. As best shown by the curves (Fig. 1) these values rise early and rapidly in the pre-ovulation period; thereafter they decrease, though they remain slightly above normal at the center of the ovulation period; they attain a minimum or normal level toward the middle of the post-ovulation period. This occurrence of a maximum change *early* in the pre-ovulation period is of considerable interest, since all of the other cyclic changes already described attain a maximum at a definitely *later* period—namely, coincident with the act of ovulation itself. The known and suspected relations of the ovary to the metabolism of fats and lipoids, together with this early attainment of maximum values for these particular substances in the blood, suggest that it is the ovary—rather than other organs thus far studied—which supplies the initiative for the entire series of observed changes at reproduction. As noted above, the function of the ovary is possibly under the guidance or control of the anterior lobe of the pituitary body.

This increase of alcohol-soluble substance in the blood supplies some reason for suspecting a diminished oxidation of fat in the tissues at the time of its most notable accumulation in the blood. At this period the constituents of fat and lecithin are certainly being removed faster from the blood by the rapidly growing ovum than during preceding days or weeks. If the rate of oxidation of fats is undisturbed it is entirely certain that an increased mobilization of fat occurs early in the pre-ovulation period. It is also of interest that female birds—perhaps the females of many animals—normally have higher concentrations of blood fat than have males; that this change in blood fat at ovulation serves to increase this sexual difference; and finally, that this difference in blood fat is probably associated with a lower basal metabolism in females than in males.

Other Metabolic Changes

The Manoilov sex reaction applied to the blood of pigeons in various reproductive stages has provided some results of

considerable interest to our present problem. When properly controlled comparisons are made between the blood of males and females the reaction gives a high proportion (85 per cent.) of correct diagnoses of the sex in reproductive stages other than at ovulation and during the first 12 days of incubation.[1] During these latter periods, however, the Manoilov test gives a notably high proportion (61 and 62 per cent.) of incorrect diagnoses of the sexuality of the birds compared. These results appear to be associated with the fact that the activity, and presumably the metabolism, of the male is greatly reduced at these periods in conformity with his habit and disposition to sit on the nest during the day-light hours. Quantitative values assigned to the grade of color present at the end-point of each reaction enabled us to learn that in one and the same male these values during incubation approached the normal female values; and, of special importance to the present topic, the female values *at ovulation* approach the normal male values. These results are interpreted as indicating, first, that the Manoilov test is a better one for metabolic level than for sex; second, that the metabolism of the female is increased at the ovulation period—the period covered by the several specific metabolic changes already considered in this paper.

Since a knowledge of the basal metabolism of the female pigeon at ovulation and non-ovulation stages is of special importance to the present topic, we will be asked concerning information on this subject. In reply it can be said that data on this point are being accumulated in connection with a collaborative study with Prof. F. G. Benedict on the metabolism of pigeons. A complication arises in the study of this point from the circumstance that the preliminary fasting and preparation of the pigeon for measurement of her basal metabolism sometimes interferes with normal ovulation—the ova going into the body cavity instead of into the oviduct. In the 15 ring doves thus far measured at ovulation we find little

[1] Riddle, O., and Reinhart, W. H., *Proc. Soc. Exp. Biol. and Med.*, Vol. XXIV., p. 359 (1927).

evidence of a change in the basal metabolism at this period, but the serious study of this point has only begun.

General Considerations

Our extended introductory statement makes a further discussion of these results largely unnecessary. It is fairly clear that the seasonal changes that have been studied and incidentally referred to here are of much importance to the more general question as to the way in which changes in the somatic metabolism, during repeated and continuous ovulation, is related to a progressive decrease in the metabolism of ova produced from winter to the end of summer. The seasonal changes in the thyroid clearly suggest that the reduced metabolism in the ova is paralleled by a reduced metabolism in the soma. The increase in parathyroid activity (blood calcium) increases much for an individual ovulation period and also increases slightly during this span of the seasons. The question of basal metabolic changes from winter to summer in the body or soma of the bird will be resolved by studies now being made. It is known that germinal metabolic change, independent of a similar change in the soma, can occur; whether it actually does so, in the case of the pigeon, should also receive a definite answer from studies now in progress.

Several well-marked changes in the activity of special organs, and in the metabolism of individual organic and inorganic constituents of the blood, have been found to occur in closest coincidence with the growth and separation of ova from the ovary of pigeons. Unquestionably, notable changes in the metabolism of fat and carbohydrate occur in coincidence with the ovulation cycle. Also, continuous ovulations in pigeons involve the maintenance at abnormal levels of the several values studied here. It is not yet possible to estimate the combined or total effects of these several changes in terms of changes in the basal metabolism of the soma. If, however, one or another change should later be found in the study of basal metabolism the several series of facts described here

should be of great assistance in an analysis and interpretation of that result.

Summary

In female doves and pigeons induced to ovulate repeatedly in rapid succession the ova normally respond by the storage of additional material. This change in the germ-cell represents a reduction in its metabolic level and is of importance to sex theory. On the question of somatic changes concurrent with ovulation it is found that a series of bodily changes begin at quite the same time that one of the many small ova suddenly attains a new rate of growth (perhaps 26 times former rate)—namely, at about 108 hours before this ovum is released from the ovary. These bodily changes continue during a further 44-hour period which intervenes before a second ovum is ovulated; and some of them require for their decline to normality an additional 108 hours. Within 108 hours, or slightly more, the oviduct increases in weight by 1,000 per cent.; the suprarenals hypertrophy by a variable but notable amount; the blood sugar is raised by 20 per cent.; the blood calcium by more than 100 per cent. Within a shorter period (50 hours) the blood fat increases by 35 per cent.; and the lipoid blood phosphorus by 50 per cent. Manoilov blood tests indicate a change toward a "male" reaction, and this is tentatively interpreted as indicating an increased somatic metabolism. Changes in basal metabolism are partially investigated, with no changes yet established, but this study is to be greatly extended. Reference is made to some seasonal somatic changes which coincide with metabolic changes in the ova.

THE GASEOUS METABOLISM OF LARGE WILD BIRDS UNDER AVIARY LIFE

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From the standpoint of comparative physiology a study of the metabolism of birds is of interest because of their high rectal temperature, the high degree of insulation afforded by their usually heavy coat of feathers, and the fact that they somewhat approximate the sphere in configuration. Undoubtedly the body cells of the bird are at a much higher temperature than are those of most mammals, and the conditions for heat production and heat loss are noticeably different from those with hairless animals like the elephant, the rhinoceros, the hippopotamus, and man, or even the ordinary domestic fur-covered animals.

Gaseous metabolism measurements on birds have been confined usually to domestic fowl and pigeons, and the instances in which wild birds have been studied, especially large wild birds, are relatively few. Since the research to be reported in this paper deals with wild and not with domestic birds, a review of the literature on the gaseous metabolism of birds may be limited to those publications reporting measurements on wild birds.

With the recent increase in interest in the insensible perspiration not only of humans but of domestic animals,[1] unusual historic interest attaches to the work of Erasistratus [2]

[1] Benedict and Root, *Arch. Intern. Med.*, 1926, Vol. XXXVIII., p. 1; Benedict and Benedict, *Biochem. Zeitschr.*, 1927, Vol. CLXXXVI, p. 278; Benedict and Ritzman, Carnegie Inst. Wash., Pub. No. 377, 1927, p. 63.

[2] Cited by Heidel, Antecedents of Greek corpuscular theories, Harvard studies in classical philology, 1911, Vol. XXII., p. 138, who gives the following reference: Diels,

who, as early as the third century B.C., made observations on the insensible perspiration of hens and other fowls, although probably no wild birds were studied. The birds, after being fed and carefully weighed, were placed in a jar or in what Heidel, writing from Connecticut in the atmosphere of Wesleyan University (the home of the respiration calorimeter of Atwater), terms a "crude respiration calorimeter." After a time they were taken out and again weighed, together with the visible excreta. A rough approximation was thus obtained of the insensible perspiration, which present-day researches have shown is frequently correlated with the metabolism.

Lavoisier,[1] in his classical studies on respiration, included observations upon a sparrow. Since his time wild birds have been studied only occasionally. In 1824 Edwards [2] reported measurements of the carbon-dioxide production and the oxygen consumption of several sparrows and ten adult yellow hammers. Since his data did not include information regarding food and activity, however, one can draw no conclusions except that made by Edwards that the gaseous metabolism of birds is variable according to their age, species, and individual peculiarities.

In 1824 Despretz [3] also published his series of observations on animal heat. In addition to domestic fowl, several wild birds were studied, including a large Virginian owl, four screech owls, and four magpies which had been fed with meat. Dulong,[4] who had competed for the Paris Academy prize awarded to Despretz, deposited his results with the Academy in 1822, but they were not printed until after his death in 1843. Among the animals studied by him was one bird—a kestrel.

Anonym. London, 33, 43: *πρὸς δὲ τούτοις καὶ Ἐρασίστρατος πειρᾶται κατασκευάζειν τὸ προτεθέν. εἰ γὰρ λάβοι τις ζῷον οἷον ὄρνιθα ἢ τι τῶν παραπλησίων, καταθοῖτο δὲ τοῦτο ἐν λίβητι ἐπί τινας χρόνους μὴ δοὺς τροφήν, ἔπειτα σταθμήσαιτο σὺν τοῖς σκυβάλοις τοῖς αἰσθητῶς κεκενωμένοις, εὑρήσει παρὰ πολὺ ἔλασσον τοῦτο τῷ σταθμῷ τῷ δηλονότι πολλὴν ἀποφορὰν γεγενῆσθαι κατὰ τὸ λόγῳ θεωρητόν.*

[1] Lavoisier, *Mém. l'Acad. des Sci.*, 1777, p. 185; Œuvres de Lavoisier, Paris, 1862, Vol. II., p. 174.

[2] Edwards, "De l'influence des agens physiques sur la vie," Paris, 1824.

[3] Despretz, *Ann. d. Chimie et d. Physique*, 1824, 2d ser., Vol. XXVI., p. 337.

[4] Dulong, *Ann. d. Chimie et d. Physique*, 1843, 3d ser., Vol. I., p. 440.

At this early state of calorimetry the results of both Dulong and Despretz have but little more than historic interest, as large errors were inherent in their methods.

Boussingault,[1] in 1844, reported a number of measurements on turtle-doves, but all had had food and the carbon-dioxide production alone was determined. The influence of fasting and the influence of day and night were studied. A year later Letellier,[2] studying chiefly the influence of extreme differences in temperature, reported respiration experiments which included a number of birds, among them the canary, mountain sparrow, turtle-dove, green finch, and kestrel.

Lehmann,[3] in 1846, gave the results of his measurements of the carbon-dioxide production of wild birds, including the rock pigeon and the finch. Special emphasis was laid upon the effect of the environmental temperature (which was altered from 1° to 37° C.) and the question of humidity in the air. Lehmann's findings should have influenced more profoundly all respiration work on birds, including our own, but apparently little note has been taken of his data and the fact that he found a distinctly lower metabolism with increasing temperatures, provided the air was not too moist, has been too long overlooked. Our own work did not take this fact into account, and some recent publications on the metabolism of pigeons likewise show complete disregard of this temperature factor. A careful study of the classics will often materially better modern research. The particular object of Lehmann's study did not necessitate that the observations should be made under conditions comparable to what are called basal at the present time. The birds were fed three hours before the experiment and hence were measured more or less at the height of digestion.

Regnault and Reiset,[4] in their study on the gaseous ex-

[1] Boussingault, *Ann. d. Chimie et d. Physique*, 1844, 3d ser., Vol. XI., p. 433.

[2] Letellier, *Ann. d. Chimie et d. Physique*, 1845, 3d ser., Vol. XIII., p. 478.

[3] Lehmann, *Abhandl. b. Begründung d. k. sächs. Gesellsch. d. Wissensch.*, 1846, p. 463 (cited by Voit, *Zeitschr. f. Biol.*, 1878, Vol. XIV., p. 66; also cited by Hidding, Ueber den Einfluss der Luftfeuchtigkeit auf die Kohlensäureproduktion des Tieres, Diss. Hannover, 1912, p. 16. Original reference not available.)

[4] Regnault and Reiset, *Ann. d. Chimie et d. Physique*, 1849, 3d ser., Vol. XXVI., p. 299.

change of animals with their closed-circuit respiration apparatus, included measurements on several small wild birds, the green finch, sparrow, and crossbill, in addition to domestic fowl. Here again the factors of activity and digestion were not sufficiently emphasized to make the values distinctly comparable. It was noted, however, that there was a greater production of carbon dioxide at a lower temperature.

In 1870 Paul Bert [1] reported observations on a sparrow, and in his data there is at least one period in which the bird was said to have been motionless. The sparrow, which weighed 25 grams, had an oxygen consumption of 467 cc. per 100 grams per hour at an environmental temperature of 30° C. The values obtained in earlier experiments can hardly be compared with our results because of the disregard of activity and digestion, and Bert's value for his sparrow, although activity is ruled out, likewise has no definite use for comparison at the present day, because no information is given with regard to the previous feeding.

In a comprehensive study of respiration and perspiration with different species of animals, Pott [2] in 1875 included determinations of the carbon-dioxide output of a female canary (*Fringilla canaria*), two male sparrows and one female sparrow (*Passer domesticus*). The experiments were made at a temperature of about 17° C. and usually were from one to two hours long. In practically all cases there was considerable activity.

Richet,[3] in 1890, issued a publication dealing exclusively with birds, including domestic fowl, the pigeon, and the goldfinch. He not only measured the carbon-dioxide production and the respiratory quotient, but also approximated the surface area by the formula $K \times w^{2/3} \times 0.8$, in which K equals 12 and 0.8 is the average density of the bird's body. Although some of the smaller birds were very active and all had received food, Richet concludes that the carbon-dioxide

[1] Bert, "Leçons sur la physiologie comparée de la respiration," Paris, 1870, p. 503.
[2] Pott, *Landw. Versuchsstationen*, 1875, Vol. XVIII., p. 81.
[3] Richet, *Arch. d. Physiol. norm. et path.*, 1890, ser. 5, Vol. II., p. 483.

excretions were proportional not to the body weight but to the surface area, and he argues that this law holds true with birds of different sizes and kinds.

Although primarily interested in the composition of expired air, Bohr, Bock, and Jacobsen,[1] in 1897, studied the respiratory metabolism of at least two web-footed sea birds, one a *Uria troile* weighing about 600 grams, and the other a *Mormon fratercula* with a body weight of approximately 350 grams.

In several reports concerning studies of the heart-rate, Miss Buchanan[2] gives values for the carbon-dioxide production and the oxygen consumption of four wild birds, as shown in Table 1.

TABLE 1

RESPIRATORY EXCHANGE OF BIRDS (MISS BUCHANAN)

BIRD	YEAR REPORTED	BODY WEIGHT	PER KG. PER HOUR	
			Carbon Dioxide	Oxygen
		gm.	gm.	gm.
Greenfinch.....	1909	27.0	11.7	8.85
Sparrow.....	1909	26.6	12.2	7.9
Canary.....	1910	20.0	11.7	11.0
Wild duck.....	1913	770	2.305	2.32

In 1919 Leichtentritt[3] reported the oxygen consumption of some sparrows and blackbirds, laying special emphasis upon new-born and very young birds. A year later Jansen and Mangkoewinoto[4] gave the results of their study of the respiratory quotient of paddy-birds fed on polished rice.

With a closed-circuit respiration apparatus in the laboratory of Kestner, Groebbels[5] studied the gaseous exchange

[1] Bohr, Bock, and Jacobsen, *Overs. Danske Vidensk. Selsk. Forh.*, Copenhagen, 1897, p. 207.

[2] Buchanan, *Journ. Physiol.*, 1909, Vol. XXXVIII., p. lxii; *ibid.*, *Science Progress*, London, No. 17, July 1910, pp. 66–67 (see also, Annual Report Smithsonian Institution, 1910, p. 492); *ibid.*, *Journ. Physiol.*, 1913, Vol. XLVII., p. iv.

[3] Leichtentritt, *Zeitschr. f. Biol.*, 1919, Vol. LXIX., p. 545.

[4] Jansen and Mangkoewinoto, *Med. geneesk. Lab. te Weltevreden*, Java, 1920, 3d ser. A, Nos. 1–3, p. 50.

[5] Groebbels, *Zeitschr. f. Biol.*, 1920, Vol. LXX., p. 477.

of birds. The significance of light and the effects of food, external temperature, and activity, were recognized in so far as the experimental procedure would permit, but relatively few observations could be said to have been made when the birds were in the strictly post-absorptive, quiet state. The largest birds studied were two hawks, weighing from 500 to 600 grams, which were 4 and 6 weeks old, respectively. Other birds studied were a 360-gram wood owl, a 330-gram carrion crow, 6 weeks old, and a number of smaller birds ranging in weight from a 17-gram canary to a 74-gram starling. The results obtained for the minimum metabolism are summarized in Table 2.

TABLE 2

HEAT PRODUCTION OF BIRDS (GROEBBELS)

BIRD	BODY WEIGHT	EXPERIMENTAL TEMPERATURE	CALORIES PER 24 HOURS	
			Per Kg.	Per Sq. M.
	gm.	° C.		
Hawk	600	20–22	93	780
Wood owl	360	21	106	730
Crow	330	20	127	840
Starling	74	22–23	270	1100
Bullfinch	23	22	940	—
Chaffinch	22	22	1000	2700
Robin	20	27	1150	3000
Canary	17	22–23	1060	2600

A marked influence of temperature was noted in that with any one bird the lower metabolism was found at the higher temperatures. Although Groebbels emphasizes the fact that the surface area law does not hold for these birds, he does not give his method for computing the body surface. But no matter what error is introduced in such a computation, it is obvious that a complete harmonization of the results per square meter of body surface could not possibly be conceived which would account for the difference in the heat production per square meter of body surface of from 730 to as high as 3000 calories, and it is clear that we have here a definite indication of a striking difference in the heat production per unit of

surface area. It is hardly conceivable that the influence either of temperature or of digestion could explain this tremendous difference, although both factors undoubtedly play a rôle. It is of significance that the value of 3000 calories found with the robin [1] was obtained at 27° C., whereas the lowest value of 730 calories with the wood owl was noted at 21° C. An extended study was also made of canaries, in which information was secured with regard to the effect of environmental temperature (which ranged from 13° to 23° C.), and the degree of activity in each experiment was noted.

Giaja, studying especially what he calls the "minimum" and "maximum" metabolism, the former at a temperature of about 30° C. and the latter usually at nearly 3° or 4° C., reports experiments not only with rats, mice, and chickens, but with a goldfinch (*Fringilla carduelis*), a shrike or butcher bird (*Lanius excubitor*), and a ring-neck turtle-dove. Special attention was not given to the question of digestion, however, and Giaja himself points out that the metabolism measured was not the basal metabolism because the birds were not post-absorptive. Moreover the activity, which is an invariable accompaniment of exposure to a low temperature with all animals, could not be uniform. For the determination of the surface area he employed the usual formula, $K \times w^{2/3}$. To secure the most exact measurement of the surface area, he placed the fresh skin of the animal on millimeter paper and calculated the area in this way. Of significance in connection with his estimations for his birds is the fact that he found it necessary to change the value of the constant K from 6.5 for a chicken of 30 grams to 11.5 for a chicken of 193 grams. For a 15-gram goldfinch he found K equal to 7.4, for a 66-gram shrike 8.69, and for a 137-gram turtle-dove 8.7. The minimum metabolism of these birds, which Giaja conservatively states is not basal owing to the fact that food was taken, is for the goldfinch 1534 calories per square meter of body surface per 24 hours, for the shrike or butcher bird 1645 calories, and for the turtle-dove 1020 calories. As an illustration of the variability

[1] This is the small European robin (*Erythacus rubecola*).

easily introduced into these calculations by changing the value of the constant K, is the fact that in Giaja's first report [1] the heat production of the turtle-dove per square meter of body surface at 30° C. is given as 764 calories, whereas in his final report [2] it is given as 1,020 calories.

Two excellent comprehensive articles on the respiratory metabolism of birds, which do not, however, contain any new data, have recently been published by Kestner and Plaut [3] and by Schulz.[4]

Experiments Made by the Nutrition Laboratory

The Nutrition Laboratory had the privilege of studying at the New York Zoölogical Park the respiratory metabolism of a number of wild birds varying considerably in size and in general configuration, and, with few exceptions, all larger than the wild birds reported in the literature. The experiments were made intermittently from June 1920 to April 1921, and it is only to be regretted that lack of funds made it impossible to continue the work further. The investigation was made possible by the coöperation of Dr. W. T. Hornaday who was at that time the Director of the Park, and the work was greatly facilitated by the invariable courtesy and helpfulness of Mr. Lee S. Crandall, the Curator of Birds. To both of these gentlemen we wish to express our deep appreciation.

The Respiration Apparatus.—The experiments were made with the standard form of the Nutrition Laboratory's universal respiration apparatus, which has been frequently described elsewhere.[5] The chamber varied in size with the size of the bird, but the principle was the same in all cases. Thus, the chamber was closed with a water seal. Air could be

[1] Giaja, *Compt. Rend. Soc. de Biol.*, 1925, Vol. XCIII., p. 648.

[2] Giaja, *Annales de Physiologie*, 1925, Vol. I., p. 596.

[3] Kestner and Plaut, Winterstein's "Handb. d. vergleich. Physiol.," 1924, Vol. II., (Part 2), pp. 1036–1064.

[4] Schulz, Oppenheimer's "Handb. d. Biochem.," 1926, 2d ed., Vol. VII., pp. 270–288.

[5] Benedict, *Am. Journ. Physiol.*, 1909, Vol. XXIV., p. 345; *ibid.*, *Deutsch. Arch. f. klin. Med.*, 1912, Vol. CVII., p. 156; *ibid.*, Abderhalden's Handb. d. biolog. Arbeitsmethoden, 1924, Abt. IV, Teil 10, p. 440.

passed through the chamber freely, and the temperature of the issuing air was carefully noted. For the larger birds it became necessary to make a somewhat extended top to the chamber. This consisted of a large galvanized-iron, rectangular box, which would drop into the water seal and yet give an extension sufficiently high to permit of studying the largest birds. With this type of apparatus the carbon dioxide produced was weighed and the oxygen consumption was measured by noting the loss in weight of a small oxygen cylinder. Tests of the apparatus by the method of burning alcohol proved that it was capable of exceedingly exact measurements.

Routine of the Experiments.—Every effort was made to secure as nearly as possible uniform conditions with regard to food and particularly muscular repose. Certain precautions were likewise taken for maintaining the environmental temperature essentially constant. The birds were fed at 2 P.M. each day, and they usually ate up all the food immediately. When a bird was to be used in an experiment, it was, as a rule, removed from the aviary the next day just before two o'clock, *i.e.*, just before feeding time, and was placed in the respiration chamber, where it remained from 2 P.M. until about 6 or 8 P.M., when the respiration experiment began. Thus it had usually been for 28 hours or even longer without food before the experiment began. The temperature of the room in which the respiration chamber was kept had been previously brought to as nearly as possible the same temperature as that at which the bird had been living in the aviary, and was held at this point throughout the respiration experiment, so that the bird did not have to experience any sudden transition from a warm to a cold temperature or *vice versa*. We purposely selected birds which had been permitted a maximum amount of exercise, for the most part in cages large enough to allow them free flight, thus hoping to minimize the effect of cage confinement which, according to Lusk,[1] causes a distinctly lower metabolism in the case of dogs. The birds were always studied in darkness. In the summer of 1920

[1] Lusk, *Journ. Biol. Chem.*, 1915, Vol. XX., p. 565.

many of the experiments were made in the daytime, although in a light-proof respiration chamber, and the birds were not perfectly quiet. In the fall of 1920 and in 1921 all the experiments were made at night, beginning at from 6 to 8 P.M. and often continuing until 10 A.M. the next day. Frequently the birds began to be restless towards the hour for sunrise, and experimental periods which had to be ruled out because of activity usually occurred at or about this time. The degree of activity or the degree of muscular repose was always controlled by a movable cage and a kymograph tracing, such as accompanies all metabolism experiments made in connection with the Nutrition Laboratory. It was impossible, under the conditions outlined above, always to secure quiet periods even in darkness, but periods of excessive activity could always be ruled out and the kymograph tracings were helpful in many instances in selecting minimum periods for comparison purposes.

Discussion of Results

A list of the birds studied, including their English and Latin designations, their habitats, and their average body weights, is given in Table 3. All were full-grown and, with the exception of the white ibis, adult birds.

In the large number of experiments made with these birds, there are enough confirmatory periods in each day's experiment to lead to a fairly accurate estimation of the basal metabolism. Undoubtedly the absolutely irreducible, minimum metabolism of these birds was not obtained in any instance, however, for although absence of food was secured and usually muscular repose was secured, the temperature was in general too low for the measurements to be considered as representing the probable minimum metabolism. In most cases the temperature of the apparatus averaged not far from 19° C., and it should be immediately recognized from the earlier work with birds that at a temperature of 30° C. or thereabouts a somewhat lower metabolism would probably have been noted, although certain of our data lead us to

TABLE 3

WILD BIRDS STUDIED

NAME	SPECIES	HABITAT	AVERAGE BODY WEIGHT
			KG.
Cassowary	*Casuarius bennetti*	Island of New Britain, east of New Guinea	17.60
Condor	*Vultur gryphus*	Andes and Ecuador, Peru, Chile and Patagonia, North of Rio Negro	10.32
Trumpeter swan	*Cygnus buccinator*	North America	8.88
Javan adjutant	*Leptoptilus javanicus*	India, Ceylon and Malay Peninsula	5.71
Jabiru	*Jabiru mycteria*	British Guiana to Argentina	5.47
Black-backed pelican	*Pelecanus conspicillatus*	Australia, Tasmania and New Guinea	5.09
Bearded vulture	*Gypaëtus barbatus*	Central Europe and Asia	5.07
Paradise crane (Stanley)	*Tetrapteryx paradisea*	South Africa	4.03
Sand hill crane	*Megalornis mexicana*	Southern half of North America	3.89
Brown pelican	*Pelecanus occidentalis*	Atlantic coasts of subtropical and tropical America	3.51
European flamingo	*Phoenicopterus antiquorum*	Southern Europe and Central Asia. In winter India and Africa	3.04
Chilian sea eagle	*Geranoætus melanoleucus*	Colombia to Chile and Patagonia	2.86
Curassow	*Crax daubentoni*	Venezuela	2.80
Black-necked screamer	*Chauna chavaria*	Venezuela and Colombia	2.62
Purple guan	*Penelope purpurascens*	Mexico to Guatemala	2.04
Mexican blue heron	*Ardea herodias lessoni*	Mexico	1.93
Mexican blue heron	*Ardea herodias lessoni*	Mexico	1.88
Mexican blue heron	*Ardea herodias lessoni*	Mexico	1.79
Mexican blue heron	*Ardea herodias lessoni*	Mexico	1.68
Great horned owl	*Bubo virginianus*	Eastern and Northern America, South to Costa Rica	1.45
Pacific gull	*Gabianus pacificus*	Eastern Australia and Tasmania	1.21
Chilian skua	*Megalestris chilensis*	Southern South America	0.97
White ibis	*Guara alba*	Southern United States to northern South America	0.94
American bittern	*Botaurus lentiginosus*	North America to West Indies and Guatemala	0.60

believe that with these birds the environmental temperature does not have so great an effect as is commonly supposed. The estimated data for the minimum periods obtained with our different birds are recorded in Table 4, and in the absence

TABLE 4

BASAL METABOLISM OF LARGE WILD BIRDS

BIRD AND AVERAGE BODY WEIGHT	DATE OF EXPERIMENT	TIME SINCE LAST FED	EXPT. TEMP.	RECTAL TEMPERATURE	HEAT COMPUTED	
					Per 24 Hrs.	Per Kg. per 24 Hrs.
		hrs.	° C.	° C.	cal.	cal.
Cassowary, 17.6 kg...	Feb. 16–17, 1921	38–42	21.2	—	519	30
	Mar. 1–2, 1921	36–42	20.8	39.2	513	29
	Average....		21.0	—	516	30
Condor, 10.32 kg.....	April 26–27, 1921	37–43	20.4	—	351	34
Trumpeter swan, 8.88 kg.....	April 7–8, 1921	—	20.1	40.1	418	47
Javan adjutant, 5.71 kg.....	July 16, 1920	23–27	25.8	39.9	337	55
	July 17, 1920	46–50	24.4	—	255	44
	July 18, 1920	68–70	24.0	38.8	230	41
	Nov. 18, 1920	25–28	16.6	39.6	316	57
	Nov. 24, 1920	23–26	20.1	39.4	278	50
	Dec. 9, 1920	21–27	18.6	40.2	421	75
	Dec. 18, 1920	25–27	17.8	39.7	327	58
	Dec. 18–19, 1920	35–40	17.6	—	291	51
	Average....		20.6	39.6	307	54
Jabiru, 5.47 kg.....	Nov. 20, 1920	24–26	16.9	40.1	313	57
	Dec. 5, 1920	26–28	18.3	39.1	239	43
	Dec. 6, 1920	49–51	16.6	—	253	46
	Dec. 21–22, 1920	36–42	20.6	—	291	53
	Dec. 29, 1920	25–27	19.1	40.5	305	55
	Dec. 29–30, 1920	33–38	19.0	—	269	49
	Mar. 22–23, 1921	37–40	13.6	40.5	231	45
	Average....		17.7	40.1	272	50
Black-backed pelican, 5.09 kg.....	Mar. 29–30, 1921	34–42	16.3	40.5	398	77
	April 12–13, 1921	35–38	17.3	40.4	351	70
	Average....		16.8	40.5	375	74
Bearded vulture, 5.07 kg.....	July 21, 1920	47–51	25.8	—	260	50
	Nov. 4, 1920	73–75	18.4	(39.7, Nov. 3)	217	41
	Nov. 5, 1920	96–97	18.6	—	211	42
	Nov. 6, 1920	116–120	16.7	38.8	206	42
	Apr. 5–6, 1921	34–41	21.3	40.6	246	49
	Average....		20.2	39.7	228	45
Paradise crane (Stanley), 4.03 kg.....	Feb. 9–10, 1921	36–40	19.4	40.3	223	55
	Feb. 14–15, 1921	41–45	20.7	40.1	223	55
	Feb. 19, 1921	36–41	18.0	40.9	215	54
	Mar. 3–4, 1921	34–41	19.5	40.6	217	54
	Average....		19.4	40.5	220	55

TABLE 4—*Continued*

BASAL METABOLISM OF LARGE WILD BIRDS

BIRD AND AVERAGE BODY WEIGHT	DATE OF EXPERIMENT	TIME SINCE LAST FED	EXPT. TEMP.	RECTAL TEMPERATURE	HEAT COMPUTED	
					Per 24 Hrs.	Per Kg. per 24 Hrs.
		hrs.	° C.	° C.	cal.	cal.
Sand hill crane, 3.89 kg.	June 29, 1920	23–28	26.3	40.8	192	48
	June 30, 1920	46–52	25.9	—	161	41
	July 1, 1920	68–73	25.1	40.8	152	40
	Average		25.8	40.8	168	43
Brown pelican, 3.51 kg.	Jan. 11–12, 1921	33–36	19.0	42.2	268	76
	Jan. 16–17, 1921	12–14	19.9	41.2	264	75
	Jan. 18–19, 1921	35–37	17.4	41.3	260	75
	Average		18.8	41.6	264	75
European flamingo, 3.04 kg.	Nov. 16, 1920	23–25	16.3	40.3	196	66
	Dec. 10, 1920	24–26	19.4	39.1	246	80
	Dec. 10–11, 1920	35–37	18.4	—	191	63
	Dec. 26, 1920	22–26	16.8	41.1	244	80
	Dec. 26–27, 1920	30–36	17.3	—	198	65
	Average		17.6	40.2	215	71
Chilian sea eagle, 2.86 kg.	Jan. 27–28, 1921	31–41	19.4	39.9	115	40
	Jan. 30, 1921	25–28	18.5	40.3	96	34
	Jan. 31, 1921	38–45	17.8	—	100	35
	Feb. 1–2, 1921	32–36	17.5	40.3	107	37
	Feb. 6, 1921	21–27	17.4	40.1	119	42
	Feb. 7, 1921	37–43	17.9	—	103	36
	Feb. 12–13, 1921	32–40	18.1	41.0	104	36
	Average		18.1	40.3	106	37
Curassow, 2.80 kg.	Mar. 5–6, 1921	31–34	18.8	40.3	134	48
	Mar. 19–20, 1921	33–40	13.8	40.7	176	63
	Mar. 27–28, 1921	30–35	19.9	40.7	137	48
	Average		17.5	40.6	149	53
Black-necked screamer, 2.62 kg.	Mar. 31–Apr. 1, 1921	38–43	20.8	40.8	131	50
	April 3–4, 1921	36–41	18.1	40.8	142	54
	April 14–15, 1921	37–44	18.9	40.6	152	57
	Average		19.3	40.7	142	54
Purple guan, 2.04 kg.	April 10–11, 1921	33–43	17.3	40.8	112	54
Mexican blue heron, 1.93 kg.	Dec. 15, 1920	25–28	18.9	39.6	179	93
	Dec. 15–16, 1920	33–39	18.5	—	144	75
	Average		18.7	39.6	162	84

TABLE 4—*Continued*

BASAL METABOLISM OF LARGE WILD BIRDS

BIRD AND AVERAGE BODY WEIGHT	DATE OF EXPERIMENT	TIME SINCE LAST FED	EXPT. TEMP.	RECTAL TEMPERATURE	HEAT COMPUTED	
					Per 24 Hrs.	Per Kg. per 24 Hrs.
		hrs.	° C.	° C.	cal.	cal.
Mexican blue heron, 1.88 kg.	Mar. 10–11, 1921	31–39	12.6	38.9	128	70
	Mar. 15–16, 1921	33–38	15.1	39.0	122	65
	Mar. 24–25, 1921	31–37	18.2	38.9	112	58
	Average		15.3	38.9	121	64
Mexican blue heron, 1.79 kg.	June 22, 1920	24–28	22.9	—	154	82
	June 23, 1920	44–49	21.9	—	130	73
	June 24, 1920	70–74	22.7	39.9	119	70
	Average		22.5	—	134	75
Mexican blue heron, 1.68 kg.	Nov. 17, 1920	25–28	17.0	39.3	127	77
	Nov. 23, 1920	25–27	17.3	39.6	121	75
	Dec. 7, 1920	24–26	17.6	39.3	92	53
	Average		17.3	39.4	113	68
Great horned owl, 1.45 kg.	June 8, 1920	23–26	18.1	39.2	115	79
	June 9, 1920	46–49	19.2	—	99	68
	June 10, 1920	21–24	19.6	40.3	110	76
	Average		19.0	39.8	108	74
Pacific gull, 1.21 kg.	June 15, 1920	25–28	22.9	41.2	143	114
	June 16, 1920	19–23	24.3	—	121	100
	June 17, 1920	41–43	23.3	41.2	117	101
	Average		23.5	41.2	127	105
Chilian skua, 0.97 kg.	June 12, 1920	17–20	23.1	(41.2, June 13)	102	103
	June 14, 1920	65–70	20.2	40.4	94	100
	Average		21.7	—	98	102
White ibis, 0.94 kg.	July 10, 1920	22–27	24.8	40.2	86	89
	July 11, 1920	46–51	25.1	—	88	94
	July 12, 1920	69–76	25.2	40.8	81	89
	Average		25.0	40.5	85	91
American bittern, 0.60 kg.	Nov. 10, 1920	21–26	16.9	(39.5, Nov. 9)	63	99
	Nov. 11, 1920	47–51	15.6	—	54	88
	Nov. 12, 1920	70–73	14.2	—	53	89
	Nov. 13, 1920	86–96	12.8	—	53	92
	Average		14.9	—	56	92

of measurements at the so-called "critical temperature" these data may be looked upon only as a general contribution to the physiology of wild birds under aviary life, without food in the stomach, during muscular repose, and with a known and controlled environmental temperature. It is to be noted from Table 4 not only that temperature differences may play a rôle but that the different lengths of time without food may likewise play a rôle, for in a number of instances the birds fasted for a considerable length of time, the bearded vulture, for example, for 120 hours. The effect of temperature and the effect of fasting will be subsequently discussed.

Rectal Temperature.—The rectal temperature of these birds, as recorded in Table 4, is generally uniform at not far from 40° C. The lowest value is 38.8° C., noted both with the Javan adjutant on July 18 and with the bearded vulture on November 6; the maximum is 42.2° C. with the brown pelican on January 11–12. Of the average rectal temperatures, the highest is 41.6° C., observed on 3 days with a brown pelican weighing 3.51 kg., and the lowest is 38.9° C., the average of 3 days with a Mexican blue heron weighing 1.88 kg. The well-known difficulty of securing the rectal temperature of birds,[1] particularly when the element of struggle comes in, leaves these figures in doubt. We believe that they are as truly representative as any figures thus far secured, but recent work with pigeons in connection with Dr. Oscar Riddle of the Department of Genetics, at Cold Spring Harbor, Long Island, New York, has shown that previous struggle or previous flight may have a considerable effect. The rectal temperatures here recorded were taken under singularly advantageous conditions. The birds had been quiescent in the narrow confines of the respiration chamber for from 8 to 18 hours prior to the taking of the temperature, as the rectal temperature was not measured until the end of the experiment, and although there were occasionally evidences of struggle, the struggle was by no means so great as that commonly reported by other investigators as occurring prior to their measurements of rectal

[1] Riddle, *Biological Bulletin*, 1908, Vol. XIV., p. 352.

temperature. The general picture, therefore, is that the rectal temperature on the average is not far from 40° C., somewhat higher than that of mammals such as man or the dog, and about 2 degrees higher than that of the steer.

Respiratory Quotient.—Since food was withheld from the birds for at least 24 hours prior to the experiments, one would expect, *a priori*, that the respiratory quotient would approximate 0.73. In most cases this value was found. Frequently the quotient was a little below rather than above this value, so that one can assume that the birds for the most part had a respiratory quotient of from 0.72 to 0.74. With this quotient it is simple to compute the heat production from the measured carbon-dioxide production, by utilizing the calorific value of carbon dioxide at the determined respiratory quotient, usually 0.73. When the determined quotient varied from this, the corresponding calorific value of carbon dioxide was used.

Heat Production per 24 Hours.—The most important measurement made was that of the heat production, which is usually computed on the basis of 24 hours. There has been much criticism of the principle of computing the 24-hour heat production from half-hour periods of measurement, but in this research the birds were measured inside the respiration chamber usually for from six to eight hours. The birds lived free in the aviary, eating and exercising normally, and our values at least approximate the probable basal heat production of these birds during muscular repose and in the post-absorptive condition. The environmental temperature, which was constant in any given experiment, unfortunately varied between experiments and with different birds. The 24-hour heat production varies with the size of the bird, the average values ranging from 516 calories with the 17-kg. cassowary to 56 calories with the 600-gram American bittern. Roughly speaking, the heat production is lower with the smaller-weight birds, and yet this is by no means uniformly the case. The data in Table 4 are arranged in the order of the decreasing body weights of the birds, and from this arrangement it can be seen that the average heat production does not decrease

uniformly with the weight. The first aberrant figure is noted with the black-backed pelican, weighing 5.09 kg., which has a total heat production of 375 calories, measurably higher than that of any of the birds preceding it in Table 4 except for the trumpeter swan and the cassowary. The next striking deviation in this relationship is found with the Chilian sea eagle, which has a total heat production of 106 calories or about one-half of that of the European flamingo of essentially the same weight. The last striking deviation is with the Mexican blue heron weighing 1.93 kg., whose average heat production in two not well-agreeing periods is 162 calories, measurably higher than that of any of the other herons or the heavier birds immediately preceding it in the table.

Heat Production per Kilogram of Body Weight.—The method of comparing birds of various sizes on the basis of the 24-hour heat production is only an approximate one. It is commonly the custom, in comparing animals of various sizes, to divide the total heat production by the body weight and to consider the heat production per kilogram of body weight per 24 hours. Although it is admitted at the outset that it is illogical to assume that each kilogram of body weight would have, *per se*, the same heat-producing capacity, nevertheless this computation has been made and the results are recorded in the last column of Table 4. On this basis of comparison it is evident that whereas the total heat production for the most part varies directly with the size of the bird, the heat production per kilogram of body weight is inversely proportional to the size of the body, the smallest value being noted with the 17-kg. cassowary and the largest with birds weighing about 1 kg. or a little less. The marked irregularity in the general trend of the results is more strikingly shown in the heat production per kilogram of body weight than in the total heat production. Thus, as the body weight increases the heat production per kilogram of body weight in general decreases, except for the pronounced heat production of 74 calories in the case of the black-backed pelican, which is much greater than the heat production of the bird preceding it in the table or the

two birds following it. Another notable exception is found with the Chilian sea eagle, whose heat production of 37 calories is extraordinarily low, indeed lower than that of any bird in the series except the cassowary and the condor, which weigh from four to six times as much as the Chilian sea eagle. Comparison of the metabolism of these birds with that of man, on the basis of the heat production per kilogram of body weight per 24 hours, shows that the values for the larger birds, and, indeed, the Chilian sea eagle, approximate those noted with man, the average value for an extensive series of measurements of normal men being 25.7 calories per kilogram of body weight per 24 hours.[1]

Influence of Environmental Temperature.—By using the values for the heat production per kilogram of body weight, it is permissible to consider the influence of environmental temperature (in so far as our data will permit of comparison) and the influence of fasting upon individual birds. In the series of experiments with the Javan adjutant the heat production was 55 calories per kilogram of body weight per 24 hours on July 16, 1920, when the environmental temperature was 25.8° C., and was only 2 calories higher on November 18, 1920, when the temperature was 9 degrees lower, and yet the time since feeding was the same in both cases. The curassow had a higher metabolism at 13.8° C. than at 18.8° and 19.9° C. The Mexican blue heron weighing 1.88 kg., in three experiments in which the environmental temperature varied from 18.2° to 12.6° C., had a considerably higher metabolism at the lower temperature. It is highly probable, therefore, that there is a tendency for an increased metabolism with a low temperature. The evidence is by no means uniform on this point, but it does serve to accentuate our great error in not having specifically established the so-called "critical temperature" of these birds. Uniformity of conditions with regard to food and muscular repose does not relieve one of the necessity of studying birds at or a little above

[1] Harris and Benedict, Carnegie Inst. Wash. Pub. No. 279, 1919, Table C, pp. 40–43.

the so-called "critical temperature," when the minimum metabolism is sought.

Influence of Fasting.—The influence of fasting is well shown in the first three experiments with the Javan adjutant, in which, although the environmental temperature is essentially the same, the heat production per kilogram of body weight falls off perceptibly with continued fasting up to 70 hours. In the three consecutive experiments with the bearded vulture the effect of continued fasting is not pronounced, or at least it is practically offset by the slightly decreasing temperature so that the heat production is not materially altered. In the case of the American bittern the continuation of fasting from 21 to 96 hours, which would tend to lower the metabolism, is in part offset by the fall of 4 degrees in temperature, which would tend to raise the metabolism, so that in these four experiments the metabolism is practically uniform. With the sand hill crane and with the Mexican blue heron weighing 1.79 kg. a marked decrease in the heat production is noted with fasting prolonged to 73 or 74 hours. From these fragmentary data it can be concluded that with these wild birds fasting causes a marked decrease in the metabolism from the first to the third day and that with fasting prolonged beyond the third day there is a tendency for a further slight lowering in the heat production, as has been found with humans and other animals.

The Heat Production per Square Meter of Body Surface

The discussion of the metabolism of these different birds has shown that in general the largest bird has the largest total heat production and that the larger the bird the lower is the heat production per kilogram of body weight, although no definite relationship can be established between the size of the bird and the heat production per kilogram of body weight. Another method of comparing not only different animals of the same species or different species but likewise all warm-blooded animals has been that popularly based upon the

surface area law, which is in general to the effect that all warm-blooded animals have essentially the same heat production per square meter of surface area. That this general thesis has long been the subject of criticism does not lessen in the slightest its present-day popularity, and hence we are justified on this basis, if no other, in attempting to make computations in which the approximated surface areas of these birds are taken into consideration.

The original surface area conception was based upon the fact that the surfaces of two spheres are proportional to the two-thirds power of their volumes, and since with most animals volume may be considered to be the same as weight, the surface may be assumed to be in proportion to the two-thirds power of the weight. In order to express the surface area in square meters and in order to allow for variations in configuration, different constants were suggested to be employed in the formula for calculating surface areas. For birds, particularly for domestic fowl, this constant was first suggested as being 10.45.[1] Of all the common animals the bird perhaps comes nearest to representing a spherical form of body, and although there are the irregular extensions of wing and legs and neck, one could expect, *a priori*, that the formula based upon the spherical assumption would come the nearest to approximating the surface areas of birds. Based upon the experiments of other workers and a more recent revision of the methods of computing the surface area of the domestic fowl as presented by Mitchell,[2] but recognizing fully (1) that the computation or approximation of the surface area of birds is at best precarious, (2) that the great variety of configuration represented in our series of birds complicates this computation enormously, and (3) that a "constant" would not in all probability be the same for all varieties of birds or even for birds of the same species, we have assumed for purposes of comparison that the constant is 10 for all of our birds and have

[1] Rubner, *Zeitschr. f. Biol.*, 1883, Vol. XIX., p. 553.

[2] Mitchell, Card, and Hamilton, Univ. Illinois, Agric. Expt. Sta., Bull. 278, 1926, p. 80. Unfortunately Mitchell, for some unaccountable reason, stretched the skins of the birds after slaughter "as much as possible" before measuring.

computed the surface areas by multiplying the two-thirds power of the body weight by 10.[1] Obviously the weight of feathers should play some rôle, but must here be neglected. The important point is that this method of computation should give a rough approximation of the surface area and permit the estimation of the heat production per square meter of surface area. Such calculations have been made, based upon the average body weights of our birds, and the results are given in Table 5.

TABLE 5

BASAL HEAT PRODUCTION OF LARGE WILD BIRDS PER SQUARE METER OF BODY SURFACE PER 24 HOURS

BIRD	AVERAGE BODY WEIGHT	HEAT PER SQUARE METER[2]	BIRD	AVERAGE BODY WEIGHT	HEAT PER SQUARE METER[2]
	kg.	cal.		kg.	cal.
Cassowary	17.6	763	Curassow	2.80	750
Condor	10.32	741	Black-necked screamer	2.62	747
Trumpeter swan	8.88	975	Purple guan	2.04	696
Javan adjutant	5.71	961	Mexican blue heron	1.93	1,045
Jabiru	5.47	876	Mexican blue heron	1.88	794
Black-backed pelican	5.09	1,267	Mexican blue heron	1.79	909
Bearded vulture	5.07	773	Mexican blue heron	1.68	800
Paradise crane	4.03	869	Great horned owl	1.45	843
Sand hill crane	3.89	679	Pacific gull	1.21	1,119
Brown pelican	3.51	1,143	Chilian skua	0.97	1,000
European flamingo	3.04	1,025	White ibis	0.94	886
Chilian sea eagle	2.86	526	American bittern	0.60	788

In examining the data in Table 5 and recognizing that they are complicated by the fact that the temperatures were, for the most part, below the critical temperature for the birds, one sees that the heat production per square meter of body surface is by no means correlated with the body weight or the size of the bird. Thus, the heat production on this basis of measurement ranges from a minimum of 526 calories with the Chilian sea eagle (2.86 kg.) to a maximum of 1267 calories with the black-backed pelican (5.09 kg.). In the case of the black-

[1] This value also used by Lapicque and Lapicque, *Compt. Rend. Soc. de Biol.*, 1909, Vol. LXVI., p. 289.

[2] Body surface computed from formula $10 \times w^{2/3}$, in which w equals body weight in grams.

backed pelican the total heat production and the heat production per kilogram of body weight were also exceptionally large, and in the case of the Chilian sea eagle the heat production on all three bases of comparison is quite at variance with that of the other birds.

The average heat production per square meter of body surface for all 24 birds is 874 calories. Doubtless this average value will be looked upon as of considerable significance by the many adherents of the surface area law, for this is comfortingly near the 1,000 calories per square meter of body surface long considered almost as a fetish by extremists holding this conception. But if the surface area law as such is to hold, one cannot accept uncritically a variability of from 526 calories with the Chilian sea eagle to 1,267 calories with the black-backed pelican. Even if unreasonable allowances are made for errors in computing the surface area and for errors due to the much-neglected temperature factor, the situation is not greatly helped. The Chilian sea eagle and the black-backed pelican were both studied at essentially the same average environmental temperature, 18.1° C. and 16.8° C., respectively, and yet the sea eagle had the lowest and the pelican the highest heat production of all the birds. If these fairly low environmental temperatures had been elevated to the so-called "critical" point, even under these conditions it is not probable that the great difference in the metabolism of these two birds would be materially altered. Activity and digestion of food are ruled out. The average rectal temperature is essentially the same in both cases, 40.5° C. with the pelican and 40.3° C. with the sea eagle. Under these circumstances it would seem that there is no explanation for the wide difference in values, other than that we have to deal here with two entirely different heat-producing organisms in which the surface area is without the slightest significance.

The variability in the heat production even of birds of the same species, as in the case of the four Mexican blue herons, is also fairly great, although not so marked as in the case of the sea eagle and the pelican. Thus, the heat production per

square meter of body surface per 24 hours ranged from 794 calories (at 15.3° C.) to 1,045 calories (at 18.7° C.). Although there was a difference in the environmental temperature of 3.4° C., it is unlikely that the entire difference in metabolism can be attributed to this relatively small difference in temperature and the only explanation is that it is due to individual variations among the birds themselves.

It is possible that a better approximation of the true surface area might wipe out certain of these irregularities, but it is hardly conceivable that the difference between the black-backed pelican and the Chilian sea eagle could be appreciably altered by the widest permissible variations in the formula or constants involved in obtaining the surface area. It seems to us that the wide variability in the metabolism of these birds is suggestive that a further study of the chemistry of the metabolism of birds of this type is highly desirable. The low metabolism of the Chilian sea eagle can hardly have been due to the fact that the bird was moribund, for our observations extended over two weeks and the bird gave not the slightest evidence of being abnormal in any way. The same may be said with regard to the pelican. We certainly were not dealing with a febrile condition, which might produce an abnormal heat production. Problems involving the chemistry of the blood, the influence of the ingestion of food upon heat production, and the character of the excreta, suggest themselves in the study of a series of birds like these.

Summary

At the New York Zoölogical Park a study was made by the Nutrition Laboratory of the Carnegie Institution of Washington of the gaseous metabolism of large wild birds under aviary life. A closed-circuit respiration chamber connected with a universal respiration apparatus was used, and measurements were made of the carbon-dioxide production and the oxygen consumption, from which the heat production was computed. Twenty-four full-grown, adult birds were studied, ranging in weight from a 17-kg. cassowary to a 600-gram American

bittern. The results reported are based upon minimum periods of measurement during muscular repose, usually at least 28 hours or even longer after food, and at a controlled environmental temperature of about 19° C. The so-called "critical temperature" of the birds was unfortunately not determined, and the results cannot therefore be considered to represent the absolute minimum metabolism.

The rectal temperature was about 40° C., and the respiratory quotient was between 0.72 and 0.74. The 24-hour heat production varied in general with the size of the bird, ranging from 516 calories with the 17-kg. cassowary to 56 calories with the 600-gram American bittern. The heat production per kilogram of body weight was inversely proportional to the size of the bird, although there were some marked exceptions to this general rule.

The heat production per square meter of body surface per 24 hours (computed from the formula $10 \times w^{2/3}$) was on the average for all 24 birds 874 calories. It ranged from 526 calories with the Chilian sea eagle to 1,267 calories with the black-backed pelican, however. No explanation is found for the wide difference in these values other than that these birds represent two entirely different heat-producing organisms, in which the surface area is without the slightest significance. The wide variability in the metabolism of these birds is suggestive of the need for further study of the chemistry of the metabolism of birds of this type.

THE GENESIS OF ALKALINE ROCKS

By C. H. SMYTH, Jr.

(*Read April 30, 1927*)

Introduction

In an earlier paper, the writer [1] discussed the method of origin of alkaline rocks as indicated by certain rather striking features in their chemical composition, particularly the relatively large content of volatile constituents, or "mineralizers," and of rare elements. Such progress has been made, since that time, in the study of igneous phenomena, that it seems worth while to renew the discussion and inquire how far these suggestions may still be acceptable, and in what directions they demand modification. This is particularly desirable since the developments of later years seem largely to substantiate the views formerly presented.

Outline of Earlier Paper

In the paper referred to, the writer takes the position that the alkaline rocks are not, in their origin, distinct from the sub-alkaline rocks, but rather that both types are products of the differentiation of a common primary magma, the alkaline rocks requiring for their development certain favoring environmental conditions.

As to the common origin of alkaline and sub-alkaline rocks, the writer is in complete agreement with the view previously expressed by Daly [2] but differs from the latter in considering that the power to develop alkaline fractions is inherent in the primary magma and does not require the introduction of any

[1] C. H. Smyth, Jr., "The Chemical Composition of the Alkaline Rocks and its Significance as to their Origin," *Amer. Jour. Sci.* (4), Vol. XXXVI. (1913), pp. 33–46.

[2] R. A. Daly, "Origin of the Alkaline Rocks," *Bull. Geol. Soc. Am.*, Vol. XXI. (1910), pp. 87–118.

extraneous material. Despite the different orders of magnitude involved, an analogy is seen between the concentration of rare elements in pegmatites and that of similar elements in alkaline rocks, and in both cases the volatile constituents are recognized as of fundamental genetic importance, it being maintained that the concentration is

> effected through the agency of mineralizers. The power of these agents is due to their own mobility, which enables them to permeate a magma, together with their capacity to form mobile compounds with certain elements. As a result of these two properties, the mineralizers are able, under favorable conditions, to extract these elements from the magma and to transfer them elsewhere.

It is maintained also that, like pegmatites, alkaline rocks are late products of differentiation and, for this reason, are formed only when the process is carried on under such favorable conditions that these products have opportunity for relatively complete separation, yielding the entire magmatic sequence. If differentiation is in any way interrupted before these late stages are reached, no alkaline rocks are produced. This is looked upon as the reason for the frequent occurrence of alkaline rocks in relatively stable, "Atlantic," regions rather than in unstable, "Pacific," regions, since, in regions of active folding and rapid movement of magmas, the continuity of conditions necessary for the completion of the magmatic sequence is less apt to occur than in more stable regions where magmas may rest undisturbed through long periods of slow cooling. On the basis of this hypothesis, alkaline rocks are quite normal derivatives of ordinary primary magmas, and are not products of the assimilation of limestones or of saline deposits, nor are they derived from a deep-seated, primordial layer of alkaline magma, as advocated by Becke[1] and his followers.

Also, while the conditions conducive to the formation of alkaline rocks are, in any particular geological period, more pronounced in some parts of the earth than in others, so that the rocks may have a more or less sharply localized habitat,

[1] F. Becke, "Die Eruptivgebiete des böhm. Mittelgebirges und der Amerik. Andes," *Tschermak's Min. u. Pet. Mitt.*, Vol. XXII. (1903), p. 247.

they may be expected to appear sporadically in the less favorable regions.

Since an interpretation of these essential features of the paper, very different from that intended by the writer, has been made,[1] it may be well to emphasize, still further, the main thesis of the paper, that the volatile constituents, or mineralizers, have the power to develop alkaline rocks, not only in most, but presumably in all, plutonic complexes; but that this power proves effective only under especially favorable conditions. Also, it is not maintained that alkaline magmas develop where and when there is "local, quite exceptional, concentration of the mineralizers and alkalies in purely juvenile magma," but, rather, that the mineralizers and other constituents of the alkaline rocks segregate continuously and simultaneously from ordinary sub-alkaline primary magma, the mineralizers being an indispensable factor in the process, the localization being a question of conditions to which the magma is subjected throughout the entire process of differentiation. Furthermore, while the localization of alkaline rocks is recognized as being, to a considerable extent, under tectonic control, it is not maintained that alkaline magmas are products of "crustal dislocation by radial movements in the earth" but, rather, that their association with movements of this type is due to the fact that they both tend to develop in relatively stable regions. The stability, or relative absence of movement, not the kind of movement, is the important factor.

Bowen's Views

Two years after their publication, the writer's views received strong support from Bowen's[2] brilliant paper on magmatic differentiation. Treating the problem of the origin of alkaline rocks by totally different methods, Bowen arrives at conclusions in close agreement with those of the writer.

[1] R. A. Daly, "Genesis of the Alkaline Rocks," *Jour. of Geol.*, Vol. XXVI. (1918), pp. 97–134. See also "Petrography of the Pacific Islands," *Bull. Geol. Soc. of America*, Vol. XXVII. (1916), pp. 325–344.

[2] N. L. Bowen, "The Later Stages of the Evolution of the Igneous Rocks," *Jour. of Geol.*, Supplement to Vol. XXIII. (1915), No. 8, pp. 1–91.

There is the same derivation of alkaline and sub-alkaline rocks from a common parent magma, the same emphasis upon the concentration and vital importance of the volatile constituents, and the same recognition of the alkaline magmas as late products of differentiation. Bowen says, "the alkaline rocks are derivatives of the same magma as the sub-alkaline rocks and belong to a late stage of concentration of the volatile constituents of the magma" (p. 65). Again, he speaks of "great concentration of these volatile constituents" and says "many of the reactions in the magma depend upon this concentration" (p. 58), and "there comes a time under appropriate conditions when the concentration of volatile constituents brings about the production of such alkaline rocks as nepheline syenite" (p. 78).

At the same time, Bowen's theory of differentiation by crystallization, supplemented by his later work on the reaction principle,[1] affords a consistent, though not entirely complete, mechanism for the development of alkaline rocks in essential harmony with the conclusions of the writer's paper.

Modifications of the Assimilation Hypothesis

As originally presented by Daly, his assimilation hypothesis assumes limestone to be taken up by the primitive basaltic magma, setting in operation, thus early, the special conditions to which he ascribes the alkaline rocks; though he also recognizes exceptional cases in which the limestone is regarded as absorbed by more specialized magmas. Later writers, however, while adopting the hypothesis, seem, often, to regard this fundamental feature of it as unimportant, and introduce the limestone at various times.

Thus, for alkaline rocks as a whole, Shand[2] assumes an ordinary differentiation by crystallization up to the alkaline granite stage, but maintains that limestone must be added at

[1] N. L. Bowen, "The Reaction Principle in Petrogenesis," *Jour. of Geol.*, Vol. XXX. (1922), pp. 177–198.

[2] S. J. Shand, "The Problem of the Alkaline Rocks," *Proc. Geol. Soc. S. Africa* (1922), pp. xix–xxxii.

this point to bring about desilication and consequent formation of alkaline magmas.

Somewhat similar views are expressed by Foye[1] for Canadian occurrences.

Brögger[2] regards the magma of the Fen district as of alkaline affinities before assimilation began, it being in this case, however, an essexite rather than a granite. Indeed, Brögger states explicitly that, for the Fen region, "a reaction between limestone and granite, as the cause of formation of nepheline rocks, is completely excluded" (p. 374).

The implication seems to be that an alkaline tendency in the parent magma, quite independent of any assimilation, is accentuated by the addition of limestone, with the consequent development of the more strongly alkaline types. Also, like Shand, Brögger introduces a further modification of Daly's hypothesis by the acceptance of crystallization, rather than limited miscibility, as the cause of differentiation after assimilation. Indeed, he speaks of the Fen district as affording a "beautiful confirmation of Daly's, and of Bowen's (crystal settling) hypotheses."

Such modifications of the assimilation hypothesis arise, in many cases, from the necessity of adjusting it to local conditions which it fails to meet in its original form, in particular, serious difficulties often encountered in providing the necessary limestone for the process. These adjustments may be in time, in space, or in both. Brögger,[3] for example, finds it difficult to harmonize the limestone assimilation hypothesis with the age relations of the rocks, and many similar, and even more striking, cases could be named; while Geijer,[4] opposing the hypothesis, cites the Fennoscandian localities where the magma has come up through thousands of feet of granite before reaching any probable source of limestone, and

[1] W. G. Foye, "Nephelite Syenites of Haliburton County, Ontario," *Am. Jour. Sci.*, Vol. XL. (1915), pp. 413–436.

[2] W. C. Brögger, "Die Eruptivgesteine des Kristianiagebietes, IV., Das Fengebiet in Telemark, Norwegen," *Vid. Skrifter I. Mat. naturv. Kl.* (1920), No. 9, pp. 1–408.

[3] W. C. Brögger, *op. cit.*, p. 397.

[4] Per Geijer, "Problems Suggested by the Igneous Rocks of Jotnian and Sub-Jotnian Age," *Geol. Foreningens I Stockholm*, Vol. XLIV. (1922), pp. 411–443.

then only "a moderate cover," and is supposed by the assimilation hypothesis to be unaffected by the former, but profoundly modified by the latter.

Association of Alkaline Rocks with Limestone

As has often been pointed out, limestone is so widespread that its frequent occurrence in association with alkaline rocks is inevitable, and has therefore no necessary genetic significance; while, on the other hand, the occurrence of alkaline rocks remote from limestone is strong evidence that the latter is not essential to their development. Indeed, it would be final evidence but for the fact that, in most cases, it is impossible to absolutely prove the complete absence of limestone at depth, in eroded overlying formations, or at some point that might have been available if very great lateral transfer is assumed. To the writer, it appears that in some cases probabilities, or even possibilities, as to both space and time, have been stretched almost or quite to the breaking point in order to find a source for limestone; and while the intimate association between alkaline rocks and limestones is often described and greatly emphasized, numberless equally intimate associations of sub-alkaline rocks with limestones pass unnoticed.

The alkaline rocks of the Antarctic island of Kerguelen afford very striking evidence upon this question. As pointed out by Lacroix,[1] Kerguelen, unlike the province of Ampasindava, Madagascar, contains no sediments, while, in contrast with Tahiti, which is in the midst of coral formations, its latitude prohibits the existence of coral; and yet it has an assemblage of strongly alkaline rocks very similar to those of the two localities named. Even were this case unique, it would prove that it is possible for alkaline rocks to develop without limestone assimilation; and when, to this, other similar, if less striking, cases are added, together with those where limestone can be made available only by the aid of very doubtful time and space assumptions, the assimilation hy-

[1] A. Lacroix, "Mineralogie de Madagascar," Vol. III. (1924), p. 251.

pothesis seems open to grave doubts. This argument against the hypothesis is not new but, to the writer, its force seems compelling.

Unlike the case of the Kerguelen rocks, limestone assimilation is not absolutely excluded for the trachyte and the nephelite and melilite basalts of Hawaii. Daly [1] seeks the agent of their formation, from basaltic magma, in coral and foraminiferal limestone; but Cross [2] finds no actual association of alkaline rocks with limestone. Admitting, however, that "there may be superficial deposits of coral limestone about some of these islands," he raises the difficult question as to "how such material can gain access to the volcanic conduit in mass sufficient to produce any notable result." Supporting these views, Washington [3] cites the absence of limestone inclusions in the Hawaiian rocks, as contrasted with Italian volcanoes where they are abundant. He also says, "lime is one of the least variable constituents of all these Hawaiian lavas—a feature which would not be expected were the absorption of limestone one of the most important causes of differentiation." In the present writer's judgment, Hawaii is one of the many cases in which limestone assimilation, though possible, is not only far from proved, but, on any scale sufficient to materially affect the course of differentiation, is highly improbable. As Cross says, no reason is apparent for thinking that the alkaline rocks "are not, in Hawaii and elsewhere, products of the same general process of differentiation as the other rocks with which they are associated."

A more striking case than Hawaii is Ilimausak where, as Ussing [4] states, there is practically no possibility of limestone assimilation to cause the development of the great body of typical alkaline rocks. "Actual observation tells against the

[1] R. A. Daly, "Magmatic Differentiation in Hawaii," *Jour. of Geol.*, Vol. XIX. (1911), pp. 289–316.

[2] Whitman Cross, "Lavas of Hawaii and Their Relations," U. S. Geol. Survey, Professional Paper 88 (1915), p. 90.

[3] H. S. Washington, "Petrology of the Hawaiian Islands; III Kilauea and General Petrology of Hawaii," *Am. Jour. Sci.*, Vol. VI. (1923), pp. 338–367.

[4] N. V. Ussing, "Geology of the Country around Julianhaab, Greenland, Meddelelser om Gronland," Vol. XXXVIII. (1912), pp. 1–146.

hypothesis in every way." This being so, Ilimausak gives all the stronger evidence in that its rocks belong to the most highly alkaline type. The agpaites differ from ordinary nepheline syenites in the excess of alkalies in proportion to alumina, the ratio in the former being from 1.2 to 2.1, while in most nephelite syenites it is from 0.8 to 1.1. Because of the high alkalies, the abundant iron does not appear as oxide, but enters into aegirite, arfvedsonite and aenigmatite. "Even in the black kakortokite, with 25.4 per cent. iron oxides, there are no iron ores."

The order of solidification is in harmony with Bowen's views as to the ordinary line of descent for alkaline rocks, that is, in contrast with sub-alkaline rocks, the order of decreasing silica content. At Ice River, B. C., on the other hand, although conditions seem to be in many ways similar to Ilimausak, Allan [1] states very positively that the order is reversed, the more basic rocks, which here are in very small quantity, always being the first to crystallize. Unfortunately, the small number of analyses of the Ice River rocks makes it impossible to find adequate explanation for this contrast; but the presence, in the most basic variety, of large amounts of a high calcium and magnesium pyroxene, called tentatively clino-hypersthene, as contrasted with the very abundant alkaline pyroxenes and amphiboles in the basic rocks of Ilimausak, is doubtless a factor in the problem, the former probably being a higher temperature mineral and therefore, unlike the alkaline pyroxene and amphibole, separating in a relatively early stage of crystallization. The assimilation of limestone blocks, sunk to the bottom of the chamber, is of interest in this connection as a possible cause of the formation of the clino-hypersthene and the reversal of the order of crystallization. On this assumption, the limestone, instead of producing an alkaline magma, has given to such a magma a certain amount of sub-alkaline character, as indicated by the nature of the pyroxene and the order of crystallization.

[1] J. G. Allan, "Geology of Field Map-Area, B. C., and Alberta," Geological Survey, Memoir 55 (1914), p. 128.

Reactions between Alkaline Magmas and Limestone

As contrasted with many other localities, the Ilimausak rocks have little or no carbon dioxide; and this, together with the low lime and magnesia, impresses the writer as having great genetic significance. In other regions, lime, magnesia, and carbon dioxide are present, in considerable amount, in various silicates, silico-carbonates, and carbonates, which have crystallized as primary minerals from the magma. At the same time, the alkaline rocks are so intermingled with limestone, with evident assimilation, as to point to this rock as the source of these particular constituents. Such phenomena are, to many petrologists, proof that limestone assimilation is the cause of the development of the alkaline magmas.

At Ilimausak, on the other hand, where limestone is lacking, the lime and magnesia silicate and carbonate minerals are also lacking, but the formation of alkaline magmas has in no way been hindered. On the contrary, they have been formed on a large scale and with a high degree of concentration. Had these alkaline magmas come into contact with limestone, there is every reason to believe that effects would have been produced similar to those in the other regions—assimilation of limestone, formation of lime and magnesia silicate and carbonate minerals and, quite possibly, some very special rock types.

The alkaline magma, with its high concentration of water and other volatile agents, seems especially well fitted for solution of relatively large quantities of limestone and, for this reason, more liable to modification than is the sub-alkaline magma. But, in the writer's judgment, such solution is an effect, not the cause, of the special character of the alkaline magmas, and the changes induced by it are essentially those of endomorphism. This is clearly indicated in the case of Spitzkop, as contrasted with Ilimausak.

At Spitzkop, foyaite, very poor in lime, cuts granite, and develops ijolite facies, showing great increase of lime and magnesia at, and near, its contact with a large limestone xenolith. The aegirite of the foyaite gives place to diopside,

while other lime minerals develop, such as melanite, wollastonite, apatite, titanite, cancrinite, and calcite.[1] The influence of the limestone is very local; and the new minerals are, in part, of contact metamorphic type. The present writer interprets the phenomena as pointing to the solvent and reactive power of the already alkaline magma, rich in volatile constituents, rather than to the development of this magma by assimilation of limestone. The magma is not alkaline because it has assimilated limestone, but assimilates limestone because it is alkaline, and, therefore, has a high concentration of volatile constituents, great liquidity, and strong pressure.

According to this interpretation, limestone assimilation, both here and elsewhere, is a local, small-scale process, effected chiefly by the volatile constituents of alkaline magmas, which produces limited quantities of highly specialized rock types, but is unimportant in the development of alkaline rocks as a whole.

Stansfield,[2] also, suggests that the alkaline magma may dissolve much limestone and dolomite, but ascribes this action to a superheated condition of the magma. The writer finds it impossible to accept this superheated condition of the alkaline liquid, which is a residue from prolonged crystallization, and must, therefore, be of low temperature, its solvent effects being primarily due, not to excessive temperature, but to high concentration of volatile constituents and high pressure.

In quite a different connection, Von Eckerman [3] maintains that an important factor in the assimilation of limestone by granite (in contact metamorphism) is the "presence of a large percentage of alkalies and other volatile components" and Eskola [4] says that the frequent occurrence of assimilation in pegmatites shows "that the process does not require any high

[1] S. J. Shand, "The Nepheline Rocks of Sekukuniland," *Trans. Geol. Soc. S. Africa*, Vol. XXIV. (1921), pp. 144–146.

[2] J. Stansfield, "Extensions of the Monteregian Petrographical Province to the West and North-West," *Geol. Mag.*, Vol. LX. (1923), pp. 433–453.

[3] H. Von Eckerman, "The Rocks and Contact Minerals of Tennberg," *Geol. Foren.*, Vol. XLV. (1924), pp. 467–537.

[4] Pentti Eskola, "On Contact Phenomena between Gneiss and Limestone, etc.," *Jour. of Geol.*, Vol. XXX. (1922), pp. 265–294.

temperature, as the pegmatites crystallize from residual solutions, still liquid after the main igneous bodies have become solid." Though dealing with phenomena of a different order of magnitude from those concerned with the origin of alkaline magmas, these statements are in harmony with the writer's conception of the relation between such magmas and limestone assimilation.

The intimate mechanical association of alkaline rocks and limestones, that sometimes occurs, is evidence that the volatile constituents have dissolved and carried away limestone, permitting the denser part of the magma, containing inclusions of limestone that has escaped solution, to occupy the space thus afforded. This case is illustrated by the Haliburton-Bancroft nephelite syenites where, as Adams and Barlow [1] clearly show, the calcite is present as inclusions, many of which are mere remnants of larger masses derived from the adjacent limestones. In other words, here again, instead of the magma's acquiring peculiar properties through the intervention of limestone, it affects the limestone in a peculiar way because of its preëxistent peculiar properties.

The same explanation, presumably, applies to some cases of calcite in alkaline rocks, supposed to be primary and to represent assimilated limestone; that is, they are residual rather than crystallized *in situ*. In other cases, with the low temperature of basic alkaline magma and great concentration of volatile constituents, including carbon dioxide, primary calcite of purely magmatic origin may form.

The presence of vesuvianite in the nephelite syenite of Almunge, Sweden, has often been cited as proof of limestone assimilation, but the evidence is not convincing. Quensel [2] describes the vesuvianite as widely disseminated through the rock in such a way that the individual grains cannot be considered metamorphosed inclusions of limestone, but must be primary, having crystallized directly from the magma, which is supposed to have previously assimilated limestone.

[1] F. D. Adams, and A. E. Barlow, "Geology of the Haliburton and Bancroft Areas, Province of Ontario," Memoir 6, Geol. Survey Canada (1910), pp. 232–233.

[2] P. Quensel, "The Alkaline Rocks of Almunge," *Bull. Geol. Inst. Upsala*, Vol. XII. (1913–1914), pp. 124–197.

As seen by the writer, we have here a case of vesuvianite, of primary origin, crystallizing from a magma low in lime (4.43 per cent. lime, 48.60 per cent. silica); but there is no reason, whatever, apparent why this lime should be derived from assimilated limestone any more than is the lime of any other magmatic mineral. On the contrary, if the vesuvianite is a true primary mineral, as its mode of occurrence so strongly indicates, it must owe its existence, not to anything peculiar as to the source, or the quantity, of lime in the magma but, rather, to the presence of abundant water, and the relatively low crystallization temperature, the conditions, thus, approximating those of contact metamorphism, under which vesuvianite usually forms. The same reasoning applies, also, to the abundant primary cancrinite and analcite; and there is no more reason for regarding the lime of the vesuvianite and cancrinite as derived from limestone than there is for regarding the soda of the analcite as derived from the assimilation of some sodic rock. Instead, the 1.73 per cent. of combined water and the 1.10 per cent. of carbon dioxide, together with the low temperature of crystallization, point to a wholly indigenous origin for all of these minerals, leaving no reason for appeal to limestone assimilation. In general, conditions which permit of the formation of sodalite, noselite, and cancrinite, with their chlorine, sulphur trioxide and carbon dioxide, may also yield, or be closely associated with, conditions yielding vesuvianite, analcite, and calcite, with their water and carbon dioxide, as well as other relatively low temperature minerals with volatile constituents.

In an alnöite of the Monteregian province, Bowen [1] finds monticellite, melilite and biotite replacing augite and olivine, with marialite, perofskite and titaniferous magnetite as minor products. "The replacement was accomplished by an alkalic liquid (magma)." From both the field evidence and experimental data, Bowen concludes that, not only these minerals, but, also, garnet and vesuvianite, may form in alkaline magmas without the addition of lime-rich rocks.

[1] N. L. Bowen, "Genetic Features of Alnöitic Rocks at Isle Cadieux, Quebec," *Am. Jour. Sci.*, Vol. III. (1922), pp. 1–34.

Similar replacement of olivine and nephelite by phlogopite, associated with hauynite and apatite, in a Montana alnöite, points to water, fluorine, sulphur trioxide and, perhaps, chlorine, as active agents. While in part residual in the rock now exposed, Ross [1] thinks it probable that these substances were largely supplied by alkaline solutions "from deeper-seated material that was undergoing crystallization." He, further, points out the absence of limestone in the region, and the relatively low lime content in the alnöite, showing that limestone assimilation is not essential to the formation of rocks of this type, as has been so often claimed.

Another explanation for the association of alkaline rocks with limestone has recently been given by Bowen. The Fen area is characterized by a striking assemblage of alkaline rocks intimately associated with limestone, even the latter being regarded as igneous by Brögger,[2] who considers the whole complex as affording strong support for the limestone assimilation hypothesis. Bowen,[3] however, has presented a convincing argument in favor of the view that, instead of being older than, and assimilated by, the magma, the carbonates are actually younger, and replace the minerals of the alkaline rocks. Both the alkaline magmas and the replacing carbonate solutions are thought to be derived from the adjacent Christiania alkaline complex, the intensity of distribution of the replacement being in harmony with this view.

This is a new contribution to the theory of alkaline rocks which greatly weakens the limestone assimilation hypothesis by the removal of some of the chief evidence in its support; for the interpretation may apply, not only to the Fen complex, but, also, to other occurrences where the carbonates intimately associated with the alkaline rocks have been regarded by many petrologists as representing older sediments into which sub-alkaline magmas were intruded, with resultant limestone

[1] C. S. Ross, "Nepheline-Hauynite Alnöite from Winnets, Montana," *Am. Jour. Sci.*, Vol. XI. (1926), pp. 218–227.

[2] W. C. Brögger, *loc. cit.*

[3] N. L. Bowen, "The Fen Area in Telemark, Norway," *Am. Jour. Sci.*, Vol. VIII. (1924), pp. 1–11; "The Carbonate Rocks of the Fen Area in Norway," *Am. Jour. Sci.*, Vol. XII. (1926), pp. 499–502.

assimilation and conversion of the sub-alkaline into alkaline magmas. Bowen's interpretation revolutionizes this whole sequence of events; the carbonate rocks themselves become the youngest products of magmatic activity, excepting lamprophyre dike intrusion, and the alkaline rocks, instead of being products of limestone assimilation, are derived from sub-alkaline magmas in the normal course of their differentiation. The large-scale development of carbonates points to great volumes of magmatic water flowing out from the deep-seated igneous sources, the final low temperature liquid concentrate resulting from the long continued process of magmatic crystallization.

The argument of Lindgren [1] that the limestone assimilation hypothesis does not account for the concentration of chlorine, fluorine, and rare elements in the alkaline rocks is a sound one. It may even be carried farther, as the addition of limestone to basaltic magma might have just the opposite effect, and tend to precipitate some elements characteristic of strongly alkaline rocks.

When plutonic rocks cut limestones, titanium combines with calcium and is precipitated as titanite, both in the intrusive rock and in the contact zone.[2] While large segregations of titanium minerals, particularly ilmenite and titaniferous magnetite, are associated with sub-alkaline rocks, diffused titanium in augite and other minerals is characteristic of many alkaline rocks, titanium oxide often running well over 2 per cent. It would hardly seem that rocks which owe their existence to the assimilation of much limestone would show this characteristic, as the tendency would certainly be toward an elimination of titanium coincident with the assimilation of limestone. That other, rarer, elements would be similarly affected is probable.

[1] Waldemar Lindgren, "Igneous Geology of the Cordilleras; Problems of Igneous Geology," p. 276, New Haven, 1915. See also: O. H. Erdmannsdörffer, "Ueber die Bildungsweise der Erstarrungsgesteine," *Geol. Rundschau.*, Vol. VII. (1916), pp. 305–314; F. W. Clarke and H. S. Washington, "Chemistry of the Earth's Crust," Prof. Paper 127, U. S. Geol. Survey (1924), p. 107.

[2] C. H. Smyth, Jr., and A. F. Buddington, "The Geology of the Lake Bonaparte Quadrangle," Bull. N. Y. State Museum No. 269. (1926), p. 79.

Amount of Limestone Required by Assimilation Hypothesis

In a recent paper, Grout [1] has approached the assimilation hypothesis from the quantitative standpoint, and has arrived at striking results. He shows that nearly 2 per cent. of lime added to average basalt would make it less, rather than more, alkaline; and that 10 per cent. of lime, or about 23 per cent. of average limestone, is required to change a large part of the albite to nephelite. The differentiated calcic fraction would, moreover, equal in quantity the alkaline rocks produced, which, as Grout points out, is a relation very rarely found.

The conversion of a granite magma into an alkaline magma offers still greater difficulties. The calculations show that 20 per cent. of lime (45 per cent. average limestone) is insufficient to combine with all the free quartz of average granite. It would probably form an augite-rich syenite; and if the augite settled out, the magma would not be decidedly alkaline.

In the face of figures such as these, Shand's hypothesis becomes exceedingly difficult, and the qualification that he assumes alkaline, not average, granite is negligible.

Assimilation not Important in the Genesis of Alkaline Magmas

There can be little or no question that when limestone is cut by alkaline magmas, assimilation may produce some of the more highly specialized types of rocks, such as jacupirangite and ijolite, the alkaline magma being especially adapted to such action; but this is far from proving that alkaline magmas, as a whole, owe their existence to this process. Rather, as was stated in the writer's previous paper, "the magmas contain, within themselves, the essential agents of differentiation" and alkaline magmas occur "without reference to the nature of associated sediments." Beyond doubt, assimilation is a real, and often a very important, factor in magmatic development; but the available evidence does not justify the conclusion that

[1] F. F. Grout, "The Use of Calculation in Petrology," *Jour. of Geol.*, Vol. XXXIV. (1926), pp. 512–558.

it is essential to, or plays any particularly significant rôle in, the production of alkaline rocks.

Elements Concentrated in Alkaline Magmas

It was the marked concentration of volatile constituents and rare elements in alkaline, particularly sodic, rocks that led the writer to the views presented in his previous paper, such concentration pointing to the potency of the volatile constituents, a long continued process of differentiation, with segregation of the rare elements and alkalies in the final residual magmas, and relatively small volume and late solidification of these magmas to form the alkaline rocks.

Washington [1] and, later, Clarke and Washington [2] have listed the following elements as "closely and generally, if not exclusively, bound up with sodic magmas": lithium, caesium, yttrium, titanium, tin (?), fluorine, chlorine, columbium, tantalum, tungsten (?), and uranium.

Abundance and Functions of Volatile Constituents

Considerable evidence as to the abundance of volatile constituents in alkaline magmas has been given, incidentally, in the preceding pages, but a few typical examples, chosen from among a great number, will make clearer the kind of data available; while accompanying discussion of both the special cases and the more general problems will indicate the genetic significance of the association and the vital importance of the volatile constituents in the development of alkaline magmas.

The nephelite syenites of the Los Archipelago contain fluorite formed at all stages of crystallization, the new mineral villiaumite (sodium fluoride), rare fluo-silicates, sodalite, hauynite, and noselite, showing an abundance of fluorine and chlorine, with some sulphur trioxide.[3]

In the Bohemian Mittelgebirge alkaline province, evidence

[1] H. S. Washington, "The Distribution of the Elements in the Igneous Rocks," *Trans. Am. Inst. Min. Eng.*, Vol. XXXIX. (1909), pp. 735–764.

[2] F. W. Clarke and H. S. Washington, "The Composition of the Earth's Crust," Prof. Paper 127, U. S. Geol. Survey (1924), p. 107.

[3] A. Lacroix, "Les Syenites Nepheliniques de l'Archipel de Los et leurs mineraus," *Nouv. Arch. Mus.*, Ser. 5, Vol. III. (1911), pp. 1–132.

of abundance of sulphur, chlorine, and fluorine in the magmas is afforded by various sulphides, sodalite, noselite, hauynite, fluorite and fluor-apophyllite, and by the fluoritization of certain rocks and the occurrence of fluorite veins.[1]

The alkaline rocks of the Bushveld igneous complex are, also, characterized by an abundance of fluorine and chlorine, and of pneumatolytic and rare minerals, some of them of complex constitution and unstable at high temperature.[2]

The lujauritic magma of Ilimausak was very rich in volatile materials which it gave off in great quantities during crystallization; while abundant minerals of low melting point indicate crystallization at low temperature. One of Ussing's [3] analyses shows a chlorine content of 3.63 per cent. while water above 110° C. is also conspicuously abundant in the more highly alkaline varieties, reaching 4.66 per cent. in an aegirine lujaurite. Fluorine varies from low to high, but boron is absent. Carbon dioxide is lacking in most rocks, and abundant in none. In the mechanism of differentiation, Ussing emphasizes the importance of light, low temperature fractions, rich in volatile substances, while the associated alkaline pegmatites are notable for containing many very rare minerals, in which zirconium, niobium, and the cerium metals are particularly abundant.

At Ice River, B. C., development of sodalite syenite at the top of the laccolith, and of sodalite veins in the roof, the heavier ferromagnesian minerals settling toward the bottom of the chamber, points to high concentration of volatile constituents and resultant very liquid magma. Cancrinite, noselite, and fluorite are present.[4]

[1] J. E. Hibsch, "Stoff und Masse der tertiaren Eruptivgebilde des Böhmeschen Mittelgebirges," *Tschermak's Min. und Pet. Mitt.*, Bd. 35 (1922), pp. 89–110.

[2] H. A. Brouwer, "Geology of the Alkali Rocks in the Transvaal," *Jour. of Geol.*, Vol. XXV. (1917), pp. 741–778. S. J. Shand, "The Nepheline Rocks of Sekukuniland," *Trans. Geol. Soc. S. Africa*, Vol. XXIV. (1921), pp. 111–149; "The Igneous Complex of Leeuwfontein, Pretoria District," Ibidem, pp. 232–249; "The Alkaline Rocks of the Franspoort Line, Pretoria District," Ibidem, Vol. XXV. (1922), pp. 81–100.

[3] N. V. Ussing, "Geology of the Country around Julianhaab, Greenland," *Meddelelser om Gronland*, Vol. XXXVIII. (1912), pp. 1–426.

[4] J. A. Allan, "Geology of the Field Map Area, British Columbia and Alberta," Memoir 55, Geol. Survey of Canada, 1914.

The hypothesis of the derivation of the magma from a deeper-seated reservoir of basaltic magma, though linked by Allan with assimilation of stoped limestone to form alkaline magma, suggests to the writer (and, to a limited degree, Allan accepts such action) the same sort of action as in the present chamber; that is, crystallization, gravitative differentiation, with upward concentration of a light, alkaline magma having the composition of the combined magmas of the present laccolith. In virtue of its low density, high gas content, and great fluidity, the magma was able to find or make a channel through the overlying rocks to its present position, where it underwent differentiation during crystallization, with final consolidation of the highly pneumatolytic fractions. Indeed, it seems like an ideal illustration of the writer's hypothesis of the origin of alkaline rocks; and while limestone may have been assimilated to enlarge the channel, there is no sufficient reason for regarding it as essential to the production of the alkaline magma. Instead, the already alkaline magma, developed in the lower chamber, had, as already shown, marked solvent power for limestone, which helped in opening the channel; but, in view of Grout's figures, quoted above, this could hardly have affected the magma to any great extent.

The Lugar sill of teschenite, theralite, and picrite gives evidence of a magma rich in water, soda, and probably fluorine or chlorine. In the last stage of crystallization, the highly concentrated residual liquid developed large quantities of analcite, replacing earlier-formed minerals. Gravitative differentiation of the magma was made possible by the presence of the volatile constituents, which maintained a high degree of liquidity for a long time, despite the comparative thinness of the sill.

The Lugar magma, whether, as first thought by Tyrrell,[1] wholly differentiated in its present position or, as later concluded, partly differentiated in a deeper chamber, is interpreted by the present writer as one of the fractions of a larger

[1] G. W. Tyrrell, "The Late Paleozoic Alkaline Rocks of the West of Scotland," *Geol. Mag.*, Vol. IX. (1913), pp. 64–80, 120–131; "The Picrite-Teschenite Sill of Lugar (Ayrshire)," *Quart. Jour. Geol. Soc.*, Vol. LXXII. (1917), pp. 84–131.

body of less basic alkaline magma that developed, at a deeper horizon, as the residual fluid from the crystallization of a very much larger mass of magma of sub-alkaline composition. Throughout the process of crystallization, soda and water, fluorine, chlorine and other volatile constituents were steadily rejected by the growing crystals and concentrated in the residual magma. Despite falling temperature, both fluidity and gas pressure increased and finally, either through a channel formed by gas fluxing alone, or more probably by taking advantage of some small fault, the fluid fraction was strained away from the crystal fraction, intruded as a sill at a higher horizon, and there underwent crystallization differentiation with gravitative stratification.

According to this view, the Lugar magma represented a late stage in the differentiation sequence and was, doubtless, of low temperature. Without the volatile constituents it would have been too viscous to permit differentiation, even had it, as is most unlikely, been able to reach its present position at all. Indeed, as the writer sees the matter, the function of the volatile constituents is still more fundamental than is thus implied, since, in his view, without them, not only the Lugar magma, but also the larger amount of other alkaline magma from which the former is derived, would never have come into independent existence but, instead, would have remained diffused through the great body of sub-alkaline material from which it has been concentrated, and which, in the absence of the volatile constituents, would have frozen at, or before, the granite stage of the magmatic sequence. Again, however, it must be emphasized that the volatile constituents are not looked upon as having, for any reason, been present in unusual quantities in the primary magma, but instead as having undergone great concentration *pari passu* with the concentration of the other constituents of the alkaline residual fluid. In order that this may occur, it is essential that the volatile constituents should be retained in the primary magma and its liquid fractions till the alkaline stage is reached and the alkaline residual liquid separated from the sub-alkaline solid.

After this, further continuation of the same processes may yield the various alkaline fractions. Moreover, it is essential to this result that the rate of cooling be very slow, as only under such conditions can the various fractions be formed.

Evidence as to abundance and potency of volatile constituents in alkaline magmas is offered by Bowen's [1] conclusion, previously mentioned, that the carbonates, developed on a large scale in the alkaline rocks of the Fen district, instead of being derived from assimilated limestone, are of hydrothermal origin and belong to a late stage of magmatic activity in the nearby Christiania alkaline complex. The abundant carbonate solutions effecting the replacement are late, low-temperature, residues of the alkaline magmas, and (excepting a still later period of dike intrusion) the replacement is the final stage in the igneous sequence of the region.

Doubtless other localities where there is a similar association of alkaline rocks with carbonates are to be explained in the same way, adding their evidence as to the essential rôle played by the volatile constituents in alkaline magmas.

The presence of calcite as an amygdule mineral of alkaline, but not of sub-alkaline, rocks, pointed out by Holmes,[2] is of interest in this connection.

In the region under discussion, Holmes regards the alkaline and sub-alkaline rocks as derived from magmas rich in carbon dioxide,[3] and in water, respectively, but suggests the derivation of carbon dioxide from primary carbonates in very deep-seated basic rocks, rather than from assimilated limestone. The abundance of calcite in many kimberlite pipes is regarded as supporting this view.

This recalls that Daly ("Origin of Alkaline Rocks," 1910, pp. 113–114) emphasizes the probability of the juvenile character of the carbon dioxide and other gases at Cripple

[1] N. L. Bowen, "The Fen Area in Telemark, Norway," *Am. Jour. Sci.* (5), Vol. VIII. (1924), pp. 1–11; "The Carbonate Rocks in the Fen Area of Norway," Ibidem (5), Vol. XII. (1926), pp. 499–502.

[2] Arthur Holmes, "The Tertiary Volcanic Rocks of the District of Mozambique," *Quart. Jour. Geol. Soc.*, Vol. LXXII. (1916), pp. 252–279.

[3] Cf. P. Niggli, "Homogeneous Equilibria in Magmatic Melts, etc.; The Physical Chemistry of Igneous Rock Formation," *Trans. Faraday Soc.*, Vol. XX. (1925), pp. 415–501.

Creek and, in this connection, suggests that it is the presence of carbon dioxide, rather than of lime, of limestone dissolved in sub-alkaline magmas which causes the development of alkaline magmas.

The present writer would interpret the two groups described by Holmes as representing distinct parts of the differentiation sequence, the sub-alkaline group showing only early stages, the alkaline group showing the earliest stage (basalt) and late stages, the intermediate stages not having reached the surface. According to this view, the abundant carbon dioxide and the water of the alkaline magma are both due to long-continued concentration, rather than to an unusual quantity of either in the primary magma.

For the alkaline group, the writer would infer the differentiation sequence, not from the very incomplete order of eruption, but from the order of crystallization: olivine, titaniferous augite, labradorite, diopside, augite, less calcic labradorite, anorthoclase and nephelite, soda-pyroxenes, soda-amphiboles, the break in the sequence, due to lack of eruption, following the second labradorite period.

In all these cases, and in many others that might be cited, the presence of relatively large quantities of potent volatile constituents points clearly to their being essential to the formation of alkaline magmas, while the abundance of rare elements implies their concentration in a small body of residual magma, a late product of differentiation from the primary magma in which they were originally diffused. Fluorine, chlorine, sulphur trioxide, and carbon dioxide are the characteristic volatile constituents of alkaline magmas, the first-named being perhaps the most distinctive, the last by far the most abundant, in some cases, though absent in others.

In a more general discussion, Geijer [1] emphasizes the great quantities of essentially juvenile volatile constituents in nephelite syenites, and refers particularly to the frequent occurrence of these rocks in "magma-filled diatremes," seeing

[1] Per Geijer, "Problems Suggested by the Igneous Rocks of Jotnian and Sub-Jotnian Age," *Geol. For. Stockholm*, Vol. XLIV. (1922), pp. 411–443.

in this added evidence that the volatile constituents play an important part in the development of alkaline magmas.

Though accepting limestone assimilation as essential to desilication, Shand [1] agrees with the writer as to the fundamental importance of volatile constituents in the development of alkaline rocks, saying, however, that he thinks the latter develop when the magma becomes "abnormally rich in volatile constituents." The writer, on the other hand, as already stated, considers the concentration of volatile constituents a wholly normal phenomenon, when the differentiation sequence runs its entire course, without interruption.

Evans [2] has suggested that deficiency of water would cause retention of alkalies in the more basic fraction of basaltic magma, with a subsequent development of alkaline differentiates. Loss of water through normal fault fissures would thus explain the association of alkaline rocks with "Atlantic" structure.

In a later contribution, however, Evans [3] seeks the origin of alkaline magmas in the retention, in basic magmas, of the silicates of aluminium and the divalent elements as dense minerals, formed in stable regions, at depth, under heavy pressure, leaving an uncrystallized residue rich in alkalies, especially soda, and in volatile constituents. Rising through the normal fault fissures characteristic of such regions, this residual liquid produces, by further differentiation, the alkaline rocks.

Still later, Evans [4] repeats essentially the same views, emphasizing particularly the depth-pressure effect, and the association of alkaline rocks with the normal faults of stable ("tension") regions.

[1] S. J. Shand, "The Problem of the Alkaline Rocks," *Proc. Geol. Soc. S. Africa*, 1922, pp. xix–xxxii.

[2] J. W. Evans, "Discussion sur le Differentiation dans les Magmas ignes," Cong. Geol. Internat., XII. (1913), pp. 248–249; "Differentiation in Igneous Rocks," *Geol. Mag.*, Dec. 6, Vol. III. (1916), pp. 189–190.

[3] J. W. Evans, "The Origin of the Alkali Rocks," *Geol. Mag.*, Vol. LVII. (1920), p. 559.

[4] J. W. Evans, "Regions of Tension," *Quart. Jour. Geol. Soc.*, Vol. LXXXI. (1925), pp. lxxx–cxxii.

The dominant factor in this hypothesis—the direct chemical effect of high pressure—is of great interest, tending, as it does, to bring the genesis of alkaline magmas into line with many other operations of fundamental petrologic importance. The writer, however, would regard the concentration of the volatile constituents as the controlling factor in the process suggested by Evans, since, without them, no reason is apparent why the alkalies should not be fixed and retained at depth with the "divalent elements" in the form of jadeite, nephrite, and other dense alkaline minerals characteristic of the deepest metamorphic zones. Indeed, the presence of relatively large amounts of volatile constituents would seem to be the chief feature distinguishing magmatic from metamorphic conditions, at great depth, where temperature and hydrostatic pressure are uniformly high. In one case, the tendency is toward molecular adjustment without any considerable circulation of material; in the other, the volatile constituents permit the formation of mobile compounds, with resultant circulation and differentiation.

Depth-pressure control of "primary differentiation into acid and basic rocks" has been suggested by Fermor,[1] who, though his views are in many respects analogous to those of Evans, suggests that it is the elements of garnet that "migrate and leave behind a more acid residue" (p. 45), the latter containing a larger proportion of the alkalies. The alkaline rocks, as such, do not come within the scope of the brief discussion, and no mention is made of the volatile constituents; but the relation of this hypothesis to that of Evans is obvious.

Goldschmidt[2] considers sudden relief of pressure an essential factor in the production of alkaline magmas, perhaps causing the decomposition of iron-rich biotite with the resultant formation of feldspathoids and alkaline pyroxenes. In many cases, the channels of alkaline eruptions serve as outlets for the gas of large regions, and many chemical

[1] L. L. Fermor, Preliminary Note on Garnet as a Geological Barometer, etc., Records, Geol. Survey India, Vol. XLIII., Pt. I. (1913), pp. 41–47.

[2] V. M. Goldschmidt, "Stammenstypen der Eruptivgeisteine," *Vidensk. Skrift. I Mat.-Naturv. Klasse* (1922), No. 10, pp. 1–11.

peculiarities of the alkaline rocks may be determined by the presence of these gases. Here again, the volatile constituents are considered essential, and assimilation is assigned a very minor rôle. The importance ascribed to sudden loss of gases recalls a suggestion made by Bowen ("Later Stages, etc.," 1915, p. 60).

Lehmann [1] lays great stress upon differences of gas content in controlling, by consequent differences of pressure, the mineral composition of trachydolerite and tephrite derived from magmas of like composition. On the basis of molecular volumes, he concludes that the mineral association of the trachydolerites is more stable at high pressures, that of the tephrites at low pressures, and therefore that sub-alkaline rocks or alkaline rocks develop according to whether volatile constituents are retained or lost, respectively.

Washington [2] has used a similar explanation for trachydolerite and leucite rocks, though not applying it to sub-alkaline and alkaline series.

There is, in Lehmann's views, a superficial resemblance to those of Goldschmidt and, in the emphasis upon pressure, a certain similarity to the views of Evans and Fermor; but in this case appeal is made exclusively to vapor pressure, at moderate depths, rock pressure being ignored and apparently this pressure is the dominant, if not the only, effect ascribed to the volatile constituents. With this view the writer cannot agree; while the generalization as to the relation between gas content and alkaline and sub-alkaline rocks is, obviously, contrary to his entire conception of the origin of alkaline magmas. This latter disagreement, however, is apparent rather than real, being due to Lehmann's conception of the difference between alkaline and sub-alkaline rocks as purely a matter of mineral, instead of, as generally recognized, chemical, composition. The writer, and presumably most petrologists, would class all the rocks under discussion as

[1] E. Lehmann, "Differentiationsvorgänge in Natronmagma," *Neues Jahrbuch f. Min. etc.*, 1923, Bd. I., pp. 226–232.

[2] H. S. Washington, "Italian Petrographic Sketches," *Jour. of Geol.*, Vol. V. (1897), pp. 349–357.

alkaline; and if their mineral composition is controlled by varying content of volatile constituents, it is a variation within the alkaline type, and has no bearing upon the relation of the alkaline and sub-alkaline series. While the writer regards the alkaline rocks as representing magmas which, as a whole, were rich in volatile constituents, he recognizes the fact that these constituents unquestionably vary in amount and, to some extent, in character, with varying conditions, and it naturally follows that such variations have large effects.

The writer looks upon the sudden loss of volatile constituents, indicated by explosion channels, as essentially an effect, rather than a cause, of the peculiar character of the alkaline magmas, as a whole, being due to their high concentration of volatile constituents, consequent high pressure, and tendency toward violent eruption. That such relief of pressure must change the chemical equilibrium of the magma is certain, but, as in the case just discussed, it is a change within an already alkaline magma, not the development of such a magma from a sub-alkaline magma. Furthermore, with a channel open, loss of volatile constituents, decrease of pressure, and change of temperature must always change the subsequent history of the magma from what it would have been had the channel remained sealed; while gas bubbles, rising in the magma, serve as agents for the transfer of volatile compounds. An open channel is not, however, essential for the latter effect, since a cover, tight enough to resist the gas pressures in early stages of differentiation, may be permeable at the higher pressures resulting from concentration of volatile constituents with progressive differentiation. Though slower in action than an eruptive channel, such a cover may prove an effective agent in controlling the course of differentiation of alkaline magmas.[1]

Volatile Constituents as a Whole in Magmas

The behavior of the volatile constituents, as a whole, in igneous magmas has been much discussed for some years

[1] Cf. C. N. Fenner, "The Katmai Magmatic Province," *Jour. of Geol.*, Vol. XXXIV. (1926), pp. 673–772, especially pp. 743–744.

past,[1] and a concise statement in regard to it has recently been given by Morey.[2] With falling temperature and progressive crystallization, pressure rises to a maximum and then sinks, increasing concentration of volatile constituents dominating at first, temperature fall later. The pegmatite stage, temperature about 575°, is regarded as, roughly, the period of maximum pressure, which may be above 700 atmospheres with, probably, a water content of less than 20 molecular per cent.

As to composition and state, Morey says:

> As the cooling magma eliminates one compound after another by crystallization, more or less completely, the residual magma will at all times contain enough material of lesser volatility, such as the borates, phosphates, silico-fluorides, fluorides, and chlorides to hold the components of greater volatility, mainly water, in solution. In other words, the critical phenomena are never shown by the saturated solution formed by the cooling magma (pp. 294–5).
>
> By the time the residual magmatic solutions have cooled to a temperature below the critical temperature of water, the greater portion of the original magma will have crystallized. . . . If the magma is cooling under a competent crust, the non-volatile substances crystallize from a liquid containing more and more water, and at all times in the process there is a liquid phase (p. 295).

In a somewhat earlier and more detailed paper, the same author[3] gives experimental data, together with geological application, of import essentially identical with the above quotation, and lays special emphasis upon the probable effect of pressure resulting from crystallization in connection with volcanic eruption, particularly of the explosive type, associated with salic lavas.

With all of this, the writer's interpretation of the alkaline rocks, as late products of differentiation, representing residual

[1] Cf. P. Niggli, "Über Magmatische Destillationsvorgänge," *Zeits. f. Vulkan.*, Vol. V. (1919–20), pp. 61–84; and the same author's monograph, "Die Leichtflüchtigen Bestandteile im Magma," Leipsig, 1920. J. Jakob, "Zur Theorie der magmatischen Mineralisatoren," *Zeits. f. anorg. u. allg. Chemie*, Vol. CVI. (1919), pp. 229–267.

[2] G. M. Morey, "Relation of Crystallization to the Water Content and Vapor Pressure of Water in a Cooling Magma," *Jour. of Geol.*, Vol. XXXII. (1924), pp. 291–295.

[3] G. W. Morey, "The Development of Pressure in Magmas as a Result of Crystallization," *Jour. Washington Acad. Sci.*, Vol. XII. (1922), pp. 219–230.

fluids left by slow crystallization and having, therefore, an extreme concentration of volatile constituents, is in complete harmony. The volatile constituents serve to maintain a high degree of liquidity at a low temperature, holding in solution relatively large amounts of the alkalies, particularly soda, and of the various rare, accessory elements characteristic of sodic rocks. Minerals which, in a dry fusion, have fairly high melting points, are kept in solution at much lower temperatures while other, normally low-temperature, minerals are formed. Among these many, such as cancrinite, noselite, sodalite, calcite, vesuvianite, and fluorite, retain volatile constituents as essential factors in their composition.

The volatile constituents, however, not only function in this way to develop the alkaline fractions, but also are effective in promoting their mechanical separation and subsequent active circulation, in part by the maintenance of fluidity in the magmas, in part through exertion of high pressure; or, as briefly expressed in the writer's previous paper, the volatile constituents function through their capacity for combining with the elements characteristic of alkaline rocks, to "yield mobile compounds which, from their distinctive physical characters," tend to segregate as separate fractions.

Harker [1] maintains that crustal stress differences, chiefly in a horizontal sense, are the cause of separation of the alkaline magma, and Bowen [2] has supported crystal settling or squeezing out of residual liquid, though later [3] doubting its efficacy because of the relative rigidity of the materials involved. In view of what has been quoted above, the writer suggests that these extraneous agents may often play a minor rôle. Evidently, alkaline magmas must always be under the pressure of their own volatile constituents, which, in view of their temperature, must be near the maximum of, perhaps, more than 700 atmospheres assigned by Morey to the

[1] A. Harker, "Some Aspects of Igneous Action in Great Britain," *Quart. Jour. Geol. Soc.*, Vol. LXXII. (1918), pp. lxxii–xcvi.

[2] N. L. Bowen, "Crystallization Differentiation in Magmas," *Jour. of Geol.*, Vol. XXVII. (1919), pp. 373–430.

[3] N. L. Bowen, "Differentiation by Deformation," *Proc. Nat. Acad. Sci.*, Vol. VI. (1920), pp. 159–162.

pegmatite stage, and any reduction of the counterbalancing external resistance will cause a magmatic flow toward the region of such reduction. Arching, tilting, or faulting of the cover, would have this effect, and the latter, in particular, seems to be a very definite agent; while gas-boring, leading to a final explosive rupture of the thinned cover, is indicated for many chimney-shaped occurrences, such as those of Scandinavia, the South African diamond pipes, etc.

Source of Volatile Constituents

As to the source of volatile constituents, the writer can see no reason for seeking it farther than in the primary magma, from which they were concentrated throughout the whole course of differentiation. If the underlying conception that alkaline magmas are late segregations from such a magma is true, this concentration of volatile constituents is inevitable, and no reason is apparent for seeking a source in the assimilation of limestone or other rocks.

The Magmatic Sequence

Throughout this discussion, it has been assumed that the course of differentiation, starting from a primary magma, presumably of basaltic composition, passes through the subalkaline series to, and through, the alkaline series, in a continuous line, the silica content starting low, rising to a maximum, and then sinking to a second low point, the other constituents varying in harmony with this change.

While this may be accepted as a controlling principle in the development of alkaline-rocks, there can be no doubt that the sequence is much less simple than has been indicated, and that branching and variant types occur, involving highly complex relations.[1] Moreover, in the ordinary sequence of the alkaline rocks, there are often variations from, and inversions of, the usual order, which show that local or special agencies may greatly modify what may be considered the normal course of development.

[1] Cf. N. L. Bowen, "Later Stages, etc.," pp. 76–79. V. M. Goldschmidt, "Stammenstypen der Eruptivgesteine," *Vid. Skrift. I Mat.-Naturv. Kl.*, 1922, No. 10, pp. 1–12.

Tectonic Relations of Alkaline Rocks

Though frequently mentioned in the preceding pages, the problem of the importance and character of tectonic influences in the development of alkaline magmas requires further consideration.

From what has already been said, it is evident that there is much difference of opinion in regard to both phases of the problem, some petrologists practically excluding the tectonic factor, while those who regard it as important interpret its action quite variously. Most often the discussion centers about Harker's well known hypothesis of the genetic association of the two series of rocks, alkaline and sub-alkaline, with radial (Atlantic) and tangential (Pacific) crustal movements, respectively; and opinion ranges from unqualified acceptance to unqualified rejection of the hypothesis. Naturally, those who most strongly support the assimilation hypothesis are, as a rule, antagonistic to this, or any other, form of tectonic control; though such is not always the case, as some investigators see both classes of agents as working in harmony.

Faulting and Subsidence in Relation to Alkaline Magmas

Representative of the first group is Shand, whose paper,[1] already quoted, contains a re-affirmation of support of Daly's hypothesis, "in principle—if not in detail," and the following very positive statement as to tectonic relations:

> I absolutely reject the idea that movements of faulting and subsidence were in any way the *cause* of the formation of alkaline magma; that magma came into being for reasons that existed quite independently of any crustal disturbance, and it merely availed itself of any channels that were formed, and rose in them as oil or water rises in a well.

Tectonic Control and Assimilation

Brögger,[2] on the other hand, finds reason for believing that, in the Fen district, limestone assimilation and orogenic factors

[1] S. J. Shand, "The Alkaline Rocks of the Franspoort Line, Pretoria District," *Trans. Geol. Soc. S. Africa*, Vol. XXV. (1922), pp. 81–100.

[2] *Op cit.*, p. 395.

have combined to achieve the observed results. Impressed with the orientation of the occurrences of the rare types of alkaline rocks in Northern Europe parallel to the dominant trend of the Caledonian system, he concludes that the association must have a genetic significance, and recognizes a two-fold division of the eruptive rocks into a Pacific group of sub-alkaline rocks, characteristic of folded regions, and an Atlantic group of alkaline rocks, characteristic of fractured and sunken regions. Rocks of the first group occur in the folded Caledonian region, those of the second group in graben of the Christiania type.

Resistance Blocks and Folded Regions

Geijer [1] not only agrees with Brögger in recognizing tectonic control, but emphasizes it much more strongly, while minimizing assimilation. He includes in one province all the post-Archean igneous rocks of Fennoscandia, except those of the Caledonian mountain belt, which, in contrast with the others, are of sub-alkaline character. Geijer considers it "impossible to deny that the alkaline rocks are more often met with in resistance areas or regions of faulting, than in folding chains." Tectonically, the alkaline rocks are products of a slow, long-continued process of "evolution," while the sub-alkaline rocks are products of a quick, mountain-making "revolution," during which "the syntectonic intrusions . . . have moved as in a giant mixer." The character of the igneous rocks of a region depends upon the type of crustal movement, not upon its being "underlain by a magma of alkaline or sub-alkaline character." As Geijer indicates, this is all in agreement with the views expressed by the present writer.

Relief of Pressure

Goldschmidt [2] recognizes a close connection between alkaline rocks, normal faults and graben, and explosion channels.

[1] Per Geijer, "Problems Suggested by the Igneous Rocks of the Jotnian and Sub-Jotnian Age," *Geol. Foreningens i Stockholm*, Vol. XLIV. (1922), pp. 411–443.

[2] V. M. Goldschmidt, "Stammenstypen der Eruptivgesteine," *Vidensk. Skrift. I Mat.-Naturv. Klasse*, 1922, No. 10, p. 11.

Thus, tectonic control is strongly emphasized, but it is given a very special interpretation, sudden relief of pressure being regarded as its essential function in the production of alkaline magmas.

Differentiation a Function of Tectonic Type

Great emphasis is laid upon tectonic control by Niggli,[1] who recognizes three main branches of igneous rocks, Pacific (calc-alkalic), Atlantic (sodic), and Mediterranean (potassic). Within the region discussed is found an epitome of magmatic development, with rocks of all three branches. He states that the widely held conception of two primary, superimposed, magmas must be given up, and calls attention to the generally ignored fact that Becke, in a later paper,[2] modified his earlier statement that Pacific magmas are lighter than Atlantic, the fact being that density depends mainly upon ferro-magnesian content.

The type of magmatic differentiation is a function of the tectonic type with which igneous action is associated. During orogeny, the different types of differentiation and magma displacement are produced under control of external conditions. The provincial distribution of magmas is organically associated with tectonic processes. In growing mountain chains, Pacific and Mediterranean associations dominate, while in intermontane areas and forelands Atlantic rocks are commonest.

As, with passing time, orogeny becomes more restricted, older regions of folding become forelands and Atlantic rocks develop, thus becoming more widespread.

Niggli's conclusions are in essential harmony with the writer's view that alkaline rocks develop in regions of relative stability rather than in regions of active mountain-making because, in the former, continuity of conditions permits the processes of differentiation to operate gradually and thus develop the more highly specialized products. Niggli, however, does not use this interpretation.

[1] Paul Niggli, "Der Taveyannazsandstein und die Eruptivgesteine der jungmediterranean Kettengebirge," *Schweiz. Min. u. Pet. Mitt.*, Vol. II. (1923), pp. 169–275.

[2] F. Becke, *Sitzber. Akad. Wiss. Math.-Naturw. Kl.*, Bd. CXX., Abt. 1, 1911.

Plateau Intrusions

As to the tectonic relations of the Ilimausak complex, Ussing [1] says the structure is "that of a plateau area which is traversed by faults." "Folds and other structural features attributable to lateral thrust movements are entirely absent," and the discussion is concerned only with intrusives "which occur under conditions allowing of the formation of a large number of massive vertical dikes. The intrusions of South Greenland all belong to the category of plateau intrusions." Attention is called to the fact that here, as in the Christiania district, there is an association of subsidence with plutonic intrusion. Emplacement by stoping is advocated, but with little assimilation so far as the present exposed rocks are concerned.

"The batholithic invasion is in all essentials independent of the subsidence of the earth's crust, while faulting, on the other hand, may have played some part in the localization of the intrusion." A later subsidence after the intrusion is purely local and due to shrinkage on crystallization.

The author maintains that the batholith has broken through its original roof, building up a new roof of volcanic rocks, and considers this a rather frequent phenomenon. He also sharply contrasts the intrusion of "the dense gabbroid rocks" in dikes and the "specifically lighter, more acid rocks" in batholiths.

This is another case of a strongly alkaline province in a region of radial, rather than tangential, tectonic character. Structurally, as well as petrographically, its affinities are with the Christiania district, rather than the Caledonian region.

Lateral Transfer of Interstitial Liquid

Harker [2] has applied his tectonic hypothesis to an interpretation of the igneous history of Great Britain. He postulates "extensive tracts of the lower crust in a semi-liquid state, that is, as a liquid crowded with crystals or a crystalline fabric with

[1] *Op. cit.*, p. 286 *et seq.*

[2] Alfred Harker, "Some Aspects of Igneous Action in Great Britain," *Quart. Jour. Geol. Soc.*, Vol. LXXII. (1918), pp. lxxii–xcvi.

interstitial liquid." Unequal crustal stress will drive the interstitial magma "out from areas of special disturbance to accumulate beneath areas of relative quiescence. If, in addition to this lateral displacement, the magma works its way into the upper crust or to the surface, highly alkaline rock-types may result." The "areas of special disturbance" are not, as might be inferred from this quotation, regions of folding, the context showing clearly that reference is to different parts of regions of radial disturbance.

Speaking of the Lewisian plutonics, he says, "The rocks are of calcic facies, like all other rocks intruded in close connection with powerful lateral thrust."

Like Geijer and Brögger, Harker points out that the igneous rocks of the Caledonian belt of folding are sub-alkaline ("calcic") which give place to alkaline rocks outside of the folded area. Of Paleozoic igneous action as a whole, he says, "It assumed different phases and contrasted characters in accordance with the changing stress-conditions . . . during tranquil subsidence . . . the magmas extruded and intruded were invariably of alkaline nature, but calcic magmas took their place at all times of special stress."

Harker concludes that petrographic provinces are "quite definitely related to the larger displacements of the earth's crust," and "that the distribution of crustal stress is a dominant factor in determining the petrographical facies of igneous rocks."

In a slightly earlier paper, Harker [1] says: "I am wholly in accord with Bowen in the conviction that alkaline and calcic rocks are derived from the same primitive magmas." He regards the separation as being "effected on a grand scale" and "in general, in a horizontal sense," the "immediate cause" being "the action of crustal stresses squeezing out the residual fluid."

[1] Alfred Harker, "Differentiation in Intercrustal Magma Basins," *Jour. of Geol.*, Vol. XXIV. (1916), pp. 554–558.

Basic Intrusions

Views similar to those of Harker are expressed by Benson,[1] who, discussing, primarily, basic rocks, says that rocks of the essexitic-theralitic sequence occur in "stable regions of the earth's crust, . . . such as the Bohemian Mittelgebirge and other plateau areas." "Not infrequently such extrusion was associated with block-faulting, but not with folding." He mentions, also, the "very uniform series of mica-peridotite dikes of alnöitic characters . . . formed in practically unfolded regions, with perhaps fracturing and faulting of the crust, but not tangential pressure." The widespread peridotite and alnöite dikes of New York, Pennsylvania, Kentucky, and Illinois are illustrations of this type.

Marginal Relation of Alkaline Rocks to Granites

In a recent paper, Washington [2] describes the occurrence of alkaline, mainly sodic, rocks around the margins of horsts, ancient shields, and even continental regions, of average granites, citing as examples the Iberian Peninsula, the Canadian, Brazilian, and Fennoscandian shields, and the continent of Africa. This distribution, Washington says, if real, recalls Harker's hypothesis of the formation of alkaline rocks through the expulsion of residual liquid by crustal stresses. "It would not be favorable to the reference of igneous rocks to Atlantic and Pacific branches, nor to belief in the derivation of alkalic rocks from basaltic ones by assimilation of limestones."

Tension Regions

A close relation between tectonic, and rock, types is accepted, also, by Evans.[3] He thinks that the existence of a region of tension east of the North American Cordilleras, in

[1] W. N. Benson, "Tectonic Conditions Accompanying Intrusion of Basic and Ultra-Basic Igneous Rocks" (Abstract), *Bull. Geol. Soc. Amer.*, Vol. XXXI. (1920), pp. 144–148.

[2] H. S. Washington, "Granites of Central Spain," *Jour. Washington Acad. Sci.*, 16, (1926), pp. 409–412.

[3] J. W. Evans, "Regions of Tension," *Quart. Jour. Geol. Soc.*, Vol. LXXI. (1925), pp. lxxx–cxii.

Mesozoic and later times, may account for the "well known occurrences of alkali-rocks"; while the presence of alkaline rocks in eastern South America is regarded, in the absence of other evidence, as indicating another tension region. In western South Africa, the great rift-zone of East Africa, Madagascar, India, and other regions, similar associations of alkaline rocks with crustal tension and faulting are considered. Thus, Evans takes a very positive stand in support of the genetic association of alkaline rocks with regions of tension as contrasted with regions of compression, but the nature of this relation is quite different from Harker's "slow creep, mainly in a horizontal sense," of alkaline liquid derived from an intercrustal magma basin; and even more different from the present writer's conception of concentration of the alkaline and volatile constituents of ordinary sub-alkaline magmas, crystallizing at relatively moderate depths in the earth's crust, with temperature becoming quite low and gas pressure high, in the alkaline stage. With Evans, great pressure resulting from profound depth is the essential factor; with the writer, continuity of conditions, or stability, is the essential factor. There is, however, agreement in that fault fissures are looked upon as affording channels for the ascent of the alkaline magmas.

Stability and the Time Factor

As to the relation of the Ice River alkaline complex to crustal dislocation, the evidence is rather vague, but Allan [1] tentatively connects the intrusion with a fault which became the channel of upward movement. Apparently, this is one of the normal faults following the first disturbance, a moderate folding. This all fits into the hypothesis of development of alkaline magmas during periods of stability, later normal faults tapping the chamber and drawing off the light, gas-rich, mobile, alkaline magmas and leaving behind the crystallized sub-alkaline rocks.

Though not discussing the tectonic problem, Tyrrell [2]

[1] *Op. cit.*, p. 210.

[2] G. W. Tyrrell, "The Late Paleozoic Alkaline Rocks of the West of Scotland," *Geol. Mag.*, Vol. IX. (1913) p. 76.

ascribes the extreme differentiation of the Lugar sill to great liquidity maintained by volatile constituents "for a long period of time"; and, with this conclusion, it need hardly be said, the present writer is in complete agreement. The interpretations, both of this sill and of the Ice River laccolith, given on earlier pages, involve slow cooling and crystallization, with concentration of volatile constituents and consequent increasing pressure. The maintenance of these conditions for the long time required to develop the strongly alkaline magmas demands an adequate cover, and this, as well as the other conditions named, points clearly to crustal stability. Had orogenic disturbances intervened, the process would have been checked at the point then reached and, were this other than late in the sequence, the development of alkaline magmas would have been inhibited.

The contrasting development of lamprophyric residual magmas in connection with the Lausitz and Odenwald granites, discussed by Beger,[1] illustrates this type of tectonic influence, in principle, though not in detail. The Odenwald region has gabbros, basic and acid granites, and rather acid lamprophyres. Lausitz, on the other hand, has only the acid granites and more basic lamprophyres. Beger finds the explanation of this contrast in differences of cooling conditions. In the Odenwald there were many partial intrusions caused by orogenic disturbances, each one involving magma movements comparable with the thorough stirring of a liquid, which interfered with the settling of early crystals. The relatively small bodies of magma produced by these intrusions cooled quickly, lost volatile constituents, and thus became more viscous, checking differentiation much sooner than in the large body of undisturbed Lausitz magma. In the latter, a long period of slow cooling permitted relatively complete differentiation, with the final production of basic kersantites

[1] P. J. Beger, "Die Bildung des lamprophyrischen Restmagmas in Lausitz und Odenwald als Prufstein fur die Bowen'sche Differentiationshypothese," *Neues. Jahrb. f. Min. etc.*, Bd. I. (1923), pp. 220–226. P. Niggli, "Gesteins und Mineral provinzen," Bd. 1: Einführung, Lamprophyre, von P. Niggli und P. J. Beger (1923), p. 473 *et seq.*, particularly pp. 496–7.

and camptonites, in contrast with the Odenwald acid malchites.

Conditions of Solidification

As alkaline magmas are products of the last stage in the differentiation series, and for this reason can be formed only where the process can be carried well toward, or to, its limit, any conditions that cause solidification of the magma before this late stage is reached prevent the formation of alkaline fractions. Therefore, the problem of the relation of alkaline rocks to certain structural conditions and, thus, to certain localities, comes down to determining what structural conditions favor prolonged differentiation, and in what localities they occur.

As differentiation takes place only when the magma is partly liquid or, rather, during the passage from liquid to solid by progressive crystallization, it follows that the longer this condition is maintained, the better the opportunity for completion of the process. If cooling is rapid, only basic rocks will result, having the composition of the primary magma; if slower, intermediate and acid differentiates will be produced; while, if still slower, the ultimate products, alkaline rocks, will result. Slow cooling is favored by depth and by large masses of magma; thus, rapid transfer of magma up into cooler regions tends to prevent formation of alkaline rocks, unless the transferred magma is in large bodies, when, as a result of their slow cooling, after movement of the masses has ceased, alkaline satellites may be formed, as at Ice River.

A deep-seated body of magma may remain pressure-solid indefinitely, but ultimately become liquid by arching of cover or other structural relief. If the body is large, or, if smaller, at such depth as to cool slowly, gradual crystallization will give the whole series of rock types, in case the process is not interrupted by crustal disturbances. Also, crystallization will decrease the volume of the igneous mass. This, together with contraction from cooling, causes tension in the roof, which may lead to normal faulting and eruption. Sometimes, this may occur early in the sequence of differentiation, yielding

only sub-alkaline rocks; but, at other times, the strains may accumulate till the alkaline stage is reached. The controlling factors are: thickness and structure of cover, character and intensity of stresses, size and shape of igneous mass, etc. The alkaline magmas, because of their fluidity and low density tend to concentrate toward the tops of basins, leaving below a much larger fraction of the primary magma as a crystalline aggregate of sub-alkaline composition. Similarly, the alkaline magmas tend to force their way upward through the cover, the pressure of the abundant volatile constituents being an ever-present impelling force, while the fluidity of the magma is a large contributing factor. A fractured cover may serve as a strainer, retaining the fairly rigid crystal fabric, but offering upward passage to the fluid, gas-rich magma; and incipient arching may favor the separation, through the response of internal gas pressure to the lowering of external pressure. On the other hand, sagging of a fractured cover tends to squeeze together the crystal fabric, forcing upward the interstitial alkaline magma into, or through, the cover, provided the fabric is not too rigid to yield. Here dilatancy may be a factor.

Dilatancy in Connection with Tectonics

It is evident that an aggregate of interlocking, angular, and irregular crystal grains is quite different from the ideal aggregate of uniform spheres approximated by Mead's models,[1] and from the sand used in his experiments; while the water of the latter is, also, quite different from the gas-charged alkaline fluid filling the interstices between the crystal grains, but the same principle must apply.

As the condition of close packing is reached and the mass "fails along definite shear planes," these shear planes, presumably, continue upward as normal faults, and serve as channels for eruption of the very mobile interstitial magma forced out of the now rigid fabric by the pressure of its highly concentrated volatile constituents. This is quite in harmony with the state-

[1] W. J. Mead, "The Geologic Rôle of Dilatancy," *Jour. of Geol.*, Vol. XXXIII. (1925), pp. 685–698.

ment in the writer's earlier paper that "vertical fissures sometimes play an important rôle in permitting the active circulation of mineralizers through relatively rigid and, thus, but for the mineralizers, stagnant bodies of magma." The high pressure of the volatile constituents concentrated in the residual fluid is, according to this view, a vital factor in effecting the separation of the alkaline fraction from the crystal fabric, taking the place, at this stage in magmatic development, of crystal settling, or rising, and of squeezing, in the early more nearly liquid stages. Mead says (pp. 696–697):

> Plastic flow of igneous rocks must occur before cooling has developed the solid phase sufficiently to make the mass potentially dilatant. After this potentially dilatant condition of the magma has been reached, it seems reasonable to suppose that any openings or fissures in the surrounding rock will be filled, not by movement of the magma as a whole, but by the flowing out of the fluid phase under the fluid pressure of the liquid magma from the interior of the mass. This fluid phase would differ in composition from the solid phase, and consequently from the magma as a whole, by being in general more acid and containing a larger amount of mineralizers, in short, pegmatitic or aplitic in nature.

Interstitial fluid of this composition represents the granite stage of magmatic development, often the last. But when conditions permit very slow cooling and crystallization, thorough reaction, with separation of the solid, more siliceous, from the liquid, more alkaline, material, and great concentration of the volatile constituents and rare elements in the latter, the interstitual fluid becomes the magma that will form alkaline rocks, the process being, of course, a large scale one as compared with the formation of pegmatites, but analogous in principle.

Differentiation in Stable and in Unstable Regions

In brief, we have, on one hand, primary magma undergoing very slow crystallization under considerable cover, with retention of volatile constituents, and practically free from mechanical disturbances of external origin. The slow cooling favors complete separation, while the retention of volatile

constituents not only promotes fluidity and, thus, favors crystal settling, but also continues the fluid magmatic condition to the lowest possible temperature, with consequent formation of the typical alkaline fractions. Because of the high concentration of volatile constituents, these fractions, in spite of low temperature, are very fluid, and their mechanical separation is effected by gas pressure and crustal movements, the latter being of the vertical type—warping, tilting, or sagging of crustal blocks, rather than folding.

Or, on the other hand, the deep-seated magma may be forced rapidly upward, perhaps immediately after becoming liquid from a pressure-solid state, shifted from place to place, with rapidly changing conditions, and separated into a number of smaller masses. These conditions, so unlike those just described, tending to lead to quick cooling and loss of gases, render the process of differentiation crude and incomplete, checking it before the alkaline stage is reached, with consequent retention of diffused material that would form alkaline magmas if allowed to separate. This effect would most often occur in a region of strong folding, while the first described effect would be apt to occur in stable regions; so, in the folded regions, alkaline rocks would not be formed or, if formed, would, because of altitude, be quickly eroded; while in stable regions they would tend both to form, and to persist because of slow erosion. Normal faulting, affecting, perhaps, both the folded and unfolded areas, but more particularly the latter, taps deep-seated reservoirs of highly alkaline magmas, which are, thus, intruded or extruded in the unfolded area.

According to this conception, then, starting with a common primary magma, its development is different in the two types of regions, stable and unstable, and consequently yields the two different classes of rocks each exposed, in the region of its development. That the separation of the two types of structure, and of rocks, is far from complete need hardly be said, and, as Niggli (Der Taveyannazsandstein etc.) clearly shows, there are many transitional and mixed regions, both tectonically and petrographically.

Whether or not the Great Rift Valley is essentially a graben, the region is, emphatically, not one of folding, while vertical faults are frequent. Associated with these, over a great distance, we have typical alkaline rocks. This can hardly be a mere coincidence. Interpreted according to the above generalizations, the alkaline rocks of the region represent a widespread primary magma at such depth as to cool very slowly, with consequent complete differentiation. All the earlier products have separated as crystals, leaving a fluid residue, rich in alkalies and volatile constituents. Because of the high concentration of the latter, giving both pressure and fluidity, these alkaline magmas have reached the surface through the fault fissures, while the great mass of the magma is left in depth as a series of sub-alkaline rocks. If squeezing is necessary for the separation of the alkaline residual fluid from the crystal fabric, it might well be effected by the sagging of crustal blocks. The faulting may be due, in part at least, to shrinkage in volume of the crystallizing magma.

The Monteregian Hills may serve as an example of the contrasting types of differentiation associated with contrasting tectonic conditions in different parts of a region of small extent. Sub-alkaline rocks occur in the folded belt to the east, concordant in a large way with the mountain structure, as shown on geological maps. The alkaline rocks, on the other hand, occur in a belt at right angles to the folds, in the slightly disturbed Paleozoic rocks to the westward, though extending into the western border of the folds. They are strongly discordant with the mountain structures, and sharply transgressive in their relations to the sediments. The two contrasted types of rocks may be interpreted as coming from a common source, the contrast in products being due to different conditions in the two parts of the region. Beneath the Monteregian rocks must lie sub-alkaline rocks that differ in mean composition from those in the folded region only by the absence of the materials abstracted to form the alkaline rocks.

Deep and Shallow Folding

While mountain-making, in general, is adverse to the development of alkaline rocks, there are, doubtless, differences of degree in this respect.

Applying the principles developed above to Chamberlin's [1] classification of mountains as thick-shelled and thin-shelled, it would seem to follow that alkaline rocks should occur more often in the former, which are "characterized by open, gentle folding, moderate crustal shortening affecting a deeper zone," as contrasted with the intense, but shallow, folding of the thin-shelled type. In the thick-shelled type "vertical diastrophism seems to dominate over horizontal," and "normal faulting is an important accompaniment, occurring either incidentally as part of the uplifting process or as a result of subsequent relaxational movements of the raised plateau-like area."

These tectonic conditions imply large, deep-seated, relatively undisturbed, bodies of magma; and the analogy with stable regions is strengthened by the presence of normal faults, which may be of late development. The Ice River laccolith may be cited as an example of alkaline rocks formed under such conditions.

Shallow, intense folding and thrusting, on the other hand, imply conditions wholly antagonistic to the development of alkaline rocks, though such development may, of course, follow after intense mountain-making activity. As Chamberlin says, "a region which, in an earlier age, has undergone deformation of the thin-shelled type may, in a later age, after long continued denudation, participate in orogenic movements of the thick-shelled type." With increasing rigidity, due to continued intrusion and metamorphism, such a region may finally develop into a resistance block or foreland. Such a complete tectonic sequence would have, associated with it, a correspondingly complete magmatic sequence, characterized

[1] R. T. Chamberlin, "The Building of the Colorado Rockies," *Jour. of Geol.*, Vol. XXVII. (1919), pp. 145–164 and 225–251; "Vulcanism and Mountain-Making, A Supplementary Note," *Jour. of Geol.*, Vol. XXIX. (1921), pp. 166–172.

by sub-alkaline rocks in its earlier stages, and by alkaline rocks in its late stages.[1]

As a minor corollary of the foregoing, it follows that deep-seated sub-alkaline intrusions, being often syntectonic, tend toward concordant habit; while deep-seated alkaline intrusions, being commonly post-tectonic, tend toward discordant, or transgressive, habit.[2]

Direction of Transfer

While in all cases the tendency is toward an upward movement of magma, at any stage a greater or less horizontal element may be introduced. The crystal fabric is capable, to some extent, of transmitting directional stresses, but the interstitial, and expressed, fluid magma is not, and simply flows from regions of higher, to regions of lower, pressure. This, in general terms, is from deeper to shallower regions, though sometimes the horizontal movement is to be measured by many miles, as is shown by intrusive sheets. This is particularly true in the stable regions in which alkaline rocks are most frequent, where, instead of the intense crumpling of mountain regions, there are slight warpings and tiltings of crustal blocks of large horizontal dimensions. The highly fluid alkaline magma may be forced out of a region beneath the sinking part of an arch or block and transferred to the region beneath the rising part. In the case of large structural units, the horizontal element in such a transfer may be many times the vertical; but doubt may be expressed as to whether it is on such a great scale as Harker maintains, even with very slow warping, free from fracturing. The latter factor is, moreover, generally to be reckoned with; and it is because fractures of the normal fault type are frequent structural elements in stable regions that they play so important a rôle in connection with alkaline rocks. That their function is essentially that of affording channels of escape for the magma,

[1] Cf. Paul Niggli, "Der Taveyannazsandstein und die Eruptivgesteine der jungmediterranean Kettengebirge," *Schweiz. Min. u. Pet. Mitt.*, II. (1923), pp. 169–275.

[2] Cf. Per Geijer, "On the Intrusion Mechanism of the Granites of Central Sweden," *Bull. Geol. Inst. Univ. Upsala*, Vol. XV. (1916), pp. 45–60.

as maintained so strongly by Shand, the writer also believes, although others, as already pointed out, see in them a more essential rôle as affording an outlet for magmatic gases, and thus materially changing the chemical equilibrium conditions of the magma.

It is thought, then, that the rocks of an alkaline province are derived from primary magma underlying the province, rather than from magma underlying some other more or less remote region.

When a great area, like the Fennoscandian resistance block, or Geijer's "still larger unit that also embraces most of Russia," is characterized by the presence of alkaline rocks, it is hardly possible that this material has come in horizontally from some other region. Rather, these rocks must represent the residual fluid of thoroughly differentiated primary magma underlying the region, the deeper-seated fractions forming sub-alkaline rocks far below the existing surface—conditions similar to those ascribed to the Great Rift Valley region.

Again, the Mississippi Valley alnöites and mica peridotites, and the Illinois and Kentucky fluorite, are evidences of similar development of alkaline magmas, of which no large masses have reached the present surface except, perhaps, in Arkansas.

Relation to Depth of Erosion

From all this, it follows that a region of alkaline rocks may change at depth to sub-alkaline rocks, its petrographic provincial character, at a given time, depending upon the depth of erosion. A strongly folded region, on the other hand, would tend to be sub-alkaline at all horizons.

These conclusions recall Bowen's explanation of the absence of alkaline rocks from folded regions as apparent rather than real, being due to their superficial position and consequent rapid removal by erosion, in contrast with Harker's view of their absence on a genetic basis. It is evident that the writer agrees in principle with Harker, so far as the primary distribution of alkaline rocks is concerned, but, at the same time, by emphasizing the vertical rather than the

horizontal element of transfer, gives to Bowen's explanation a very wide application. That sporadic occurrences of alkaline rocks in folded regions have been removed, just as Bowen maintains, there can be no doubt. As elsewhere pointed out, Ice River is such an occurrence that has as yet escaped destruction, but erosion will ultimately leave no trace of its former presence, while the underlying sub-alkaline rocks will be exposed.

The rocks of an old, deeply eroded sub-alkaline province may, then, be the deep-seated complement of the rocks of a former alkaline province which was at a higher horizon and has been eroded away. Thus, the anorthosites of Quebec, on the one hand, and the granites and associated nephelite syenites of the Haliburton-Bancroft region, on the other, may represent igneous complexes of essentially identical character, their apparent differences resulting from the deeper erosion of the former. Sub-alkaline rocks, also, may be cut by alkaline rocks of a later period whose complementary sub-alkaline rocks are still deep buried. The Scandinavian alkaline rocks, and the Monteregian series cutting pre-Cambrian granites, west of Montreal, illustrate this. However, it is probably the commonest case, that early sub-alkaline rocks of a given igneous period are cut by later alkaline rocks of the same period and source; just as granites are cut by their associated pegmatites.

Alkaline Rocks Not Always Present in Stable Regions

To the question as to why alkaline rocks are often absent in stable regions, it may be answered that, while such regions favor the formation of alkaline rocks, the process is one of delicate adjustment of several, even many, conditions, of which depth, nature of cover, variations of these with warping, tilting, faulting, etc., time-relations of these and of other conditions, are evident. It is only when the several essential conditions exist simultaneously or in the right order of succession, and all for a sufficient length of time, that alkaline magmas develop. Also, if only plutonic alkaline masses are

formed, they may remain buried under heavy cover for an indefinite time, as, for instance, in the case of the probable deep-lying alkaline rocks of the Appalachian-Mississippi Valley region mentioned above, whose existence is indicated only by a few widely scattered peridotite and alnöite dikes, and the fluorite deposits of Illinois and Kentucky.

Conclusion

The major thesis of the writer's earlier paper is that the development of alkaline magmas "has been largely effected through the agency" of the primary volatile constituents. It is further maintained that, in so far as the development of alkaline magmas is influenced by tectonic conditions, the controlling factor is stability, as contrasted with orogenic activity.

The field and laboratory evidence accumulated during recent years, and discussed in the foregoing pages, lends strong support to both of these views, and warrants their acceptance as essential elements in present-day petrogenic theory.

The same evidence indicates, on the other hand, that the assimilation of limestone, or of other rocks, is not essential to, or even important in, the development of alkaline magmas.

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HOW HISTORY CAN BE MADE A SCIENCE

By EDWARD P. CHEYNEY

(*Read April 30, 1927*)

THE Secretaries of the Philosophical Society have recently, though probably quite unintentionally, paid to the subject of history an unwonted compliment. In a certain printed list issued a month ago they have given a classification of the present members of the Society "according to the various departments of Science which they represent," and there, along with anatomists and astronomers and geologists and physicists and others who are unquestionably scientists, are the historians—eighteen of them; 4.6 per cent. of the total membership. History is not always favored by being considered a science. A certain contemporary movement in the schools is spoken of as the teaching of "History and the Social Sciences," not "History and the other Social Sciences." As a matter of fact neither the man in the street nor the man in the study is apt to think of history as belonging with the recognized sciences. It is a form of literature or a collection of anecdotes or just a story or an appeal to patriotism or a source of arguments to be used in debate or at best a setting for the study of certain personalities or ideas or interests. Yet in this hall the question may be seriously asked whether history is really one of the sciences or not. Has the reduction of so many fields of human knowledge to scientific form and content within our time extended to history, or does this familiar subject of intellectual interest still lie outside the group of sciences? Is history capable of being treated as a science, or must it remain in a different category?

My answer to this question is (1) that the phenomena of history are as capable of being treated scientifically as

are any other objects of observation, (2) that in the hands of its most modern representatives history is fast becoming scientific, (3) that before it becomes a true science we must deliberately divest ourselves of certain habitual preoccupations concerning it. First, then, is it capable of being treated as a science? So far as its content goes the objects of historical study are all the acts and words and thoughts of men in the past. Whatever men have done or said or thought is grist to its mill; the number of the phenomena it has to deal with is therefore enormous, greater perhaps than those in any other scientific field. This number is being constantly added to, not only by accretion from current events, which from moment to moment become a part of the past, but by recovery through excavations and surveys and chance discovery of manuscripts, inscribed tablets, tomb and cave paintings, material objects, and a score of other classes of memorials of the past. Have the actions of men thus recorded been unrelated, detached occurrences, marked by variability and unrestricted self-will of the actors, or have they been, like other classes of phenomena, in some way subject to law? Can they be scientifically classified and explained or must they be forever set apart from other groups of objects of study, from chemical or botanical or astronomical or economic or mathematical phenomena by an element of voluntary choice or caprice implicit in human nature? Common observation would seem to indicate the latter alternative; that is to say that the actions of human beings are different in kind from those of inorganic and other organic substances. But "common observation" indicates that the earth is flat, and that small organisms appear by spontaneous generation. Correcting common observation by more careful thought in relation to history it seems probable that the affairs of men also have been controlled by law, much as have those of the physical and the lower organic world. The statements of the President in his opening address and many expressions used since in this hall are quite sufficient to authorize a mere student of history to assert that we live, so far as scientists know, in a unitary world.

We have seen in recent years former distinctions between plant and animal, matter and force, the organic and inorganic, the physical and the psychic, the moral and the economic, becoming constantly more tenuous. We have a growing conviction that this world is all one. The nature and the reactions upon one another of the different elements in this continuous group of phenomena that make up the observable universe are capable of discovery and logical statement. If man is a part of this world his actions are capable of being similarly discovered and described. They are matters of scientific analysis equally with any other of the subjects of study represented on our program.

Not only is the subject matter of history properly scientific material, but the methods used by historical investigators are or may well be the same as those of other scientists. Exhaustive search for material, careful observation, rigorous inference, cautious statement, ingenious hypothesis controlled by adequate later tests, are just as applicable to work in history as in zoölogy or chemistry. The materials with which the historical investigator deals are, it is true, hard to use. Observations must usually be taken at second hand, they can seldom be repeated for purposes of verification, they are often reported in vague terms, they are obscured by every kind of carelessness of statement and misrepresentation. They are often detached, but not more detached than skulls of the "dawn man." But we must, as in anthropology, do what we can with the material we have. Much can be done by detailed analysis of the sources. Everywhere in university historical seminaries and elsewhere students are being rigorously trained to handle this recalcitrant material. The historian must be modest as to the certainty or finality of his results; but his method of study of the records is not essentially different from that of the chemist with his test tube, the astronomer with his telescope or the zoölogist studying through his microscope his prepared slides. Of course this attitude toward his material is not characteristic of every historian. Mr. Froude writes: "It often seems to

me as if history was like a child's box of letters, with which we can spell any word we please. We have only to pick out such letters as we want, arrange them as we like, and say nothing of those which do not suit our purpose." But this was written half a century ago and by Mr. Froude, an historian without training in his subject, notably unsuited in temperament for historical work, and the despair of more careful historical workers even of his own time. From the modern historian's standpoint his statement is absurd.

Therefore whether the criterion be content or method there seems no theoretical reason why history should not be considered a science, and the most careful historians are ready to treat it as such. The practical matter is the third point of my thesis. What changes, if any, should be made in the habitual treatment of history to make the study and writing of it truly scientific. The remainder of this paper will be devoted to a statement of seven requirements to this end, mostly negative, but involving much positive action. In the first place, the scientific historian must learn to write strictly without partisanship or special preference; no easy task, considering his private predilections. The historian born and bred a Protestant must be able to write a history of the Reformation that will satisfy fair-minded Catholics—and *vice versa*. A history of the American Civil War must not indicate by its tone or content or conclusions whether it comes from above or below Mason and Dixon's line. An English historian must be able to write a fair history of Ireland, a Frenchman of Germany, an Italian of Austria, an American of all of them—and *vice versa*.

It has been more than two thousand years since Polybius taught us that the function of the historian is "to record with fidelity what was actually said or done, no matter how commonplace it may be." It has been a century and a half since Gibbon declared it to be the duty of History to set forth "the truth, naked unblushing truth." It has been almost a hundred years since Ranke said the past should be described "*wie es eigentlich gewesen ist*," "as it really was."

Our fellow member Henry C. Lea, the greatest of American historians, in his presidential address to the American Historical Association, defined History as "a serious attempt to ascertain the severest truth as to the past and to set it forth without fear or favor." Although practically every modern historian will acknowledge that it is his first duty to tell the truth, we have been slow to learn our lesson. Yet history, if it is to be a science, must not be written to teach any particular national, party, religious or economic doctrine. This is the first and greatest of all the historical commandments.

Secondly, teleology in historical study and writing is indefensible. The belief that God has a plan for the progress of a nation or of the world, and that this plan can be discovered by the historian and used in the explanation of events belongs in the field of religion, not of history. For centuries, from Orosius's "Seven Books of History Directed Against the Pagans" to the proposal of a modern publicist that historians should discover the way in which God has disclosed himself to man, then follow that plan in all public teaching, history has been used as the handmaid of one or another religious body; but none of this has been scientific history. History can describe the occurrences in the life of man; it is no part of its work to explain the purposes of God. For any historian to write as if he knew the plans of an all-knowing God is an unscientific as it is impious. His work lies among purely human phenomena.

Thirdly, any such notion as the "manifest destiny" of a nation or a race is mere mysticism. A nation's or a race's destiny is the result of all the influences to which it has been and is being and will be subjected. It is worked out as it goes along, not discoverable beforehand. New influences may give the history of a nation or a race a new direction at any time. Such expressions as "it was necessary that England should have a long period of separation from the Continent in order that her free institutions should be secured"; "France had to submit to despotism in order that her unity

might be attained," are unscientific conceptions. Nothing occurs in order that something else may occur. Something occurs because something else has occurred.

Fourthly, we need to be wary of attributing too much influence to individual men, no matter how great. Carlyle said that "History is the history of great men," but this was true only of Carlyle's way of writing history. It has no scientific support. We all know that in our own time the influence of even the greatest man is dependent on the existence of favoring circumstances. Mussolini is only powerful because he works in harmony with the interests and aspirations of a vast number of Italians—how many—whether a majority or a minority, whether for a few years or for a longer time; who knows? But we know that holding or expressing different opinions from the Fascisti, he would be powerless and insignificant. So it was with Napoleon, the military and political genius who embodied the national spirit of France emancipated by the Revolution. One can readily guess, if we are to make inferences at all, that fifty years earlier Napoleon would have remained a subordinate army officer, kept in relative obscurity by his plebeian origin, the fact that he served an autocratic king, and was limited by the restrictions of a long established political and social order. Queen Elizabeth was one of the most individualized of sovereigns, in the view of her contemporaries and of later ages, yet no historian will contend that she made Elizabethan England; she was merely the most conspicuous part of it. Individuals of great natural gifts or occupying high places have often given force and unity to the desires and capacities of large groups of their contemporaries, but they have not given these desires or capacities to their contemporaries, nor have they influenced appreciably the "social heritage" of their time. Their leadership has been important in matters of detail, their creative influence has been negligible. It is the difficult but unescapable duty of the scientific historian to discriminate between these two elements.

The untrained observer of the past is like a man sitting

by a mountain stream watching the chips made by the woodcutters floating by. The white chips seem to follow a self-chosen course in the stream, avoiding rocks, following swift currents and seeking quiet backwaters under the banks. They seem to follow certain leaders: a larger block comes whirling along, and in its wake follow smaller pieces as if they were dependent on its guidance or anxious to press into its train. But this is all seeming; the smaller do not really seek the larger nor do any of them, large or small, choose their own way. They are all alike swept along by currents over which they have no control and follow a course that is governed by forces beyond themselves. It is true that the analogy is not exact. The stream of human affairs is not a different stream, but is itself made up of human thoughts and actions; but none the less any individual man is as powerless to control its great currents as the drifting pieces of wood are to control those of the stream; and this is as true of the king, the general or the statesman as it is of the common man.

Fifth, military occurrences must not be given undue importance in a scientific treatment of history. The vogue of war in history is largely due to its small demands on the skill and labor of the historian. The records of wars are better preserved and more easily accessible than the sources for some other phases of history. Military events are more easy to describe than those of an economic or an intellectual order. Wars have been bound up with the early and romantic periods of a nation's history. All these causes have led to a disproportionate attention to the element of war in history. Yet the most influential incidents in the life of mankind have occurred in times of peace not of war. Not only is it true that "Peace hath her victories no less than war" but the victories—that is the achievements of mankind—in time of peace have been far more fundamental and more permanent than those in time of war. It is the duty of the scientific historian to seek out these obscure factors rather than to deal with those that are easy to write of or flattering to

national vanity. But "drum and trumpet history" is long out of fashion and there is no need to labor this point.

Sixthly, history must not be used to teach patriotism. Any ulterior object is destructive to the careful testing of materials, interpretation of events and sympathy with personalities that are essential to the scientific treatment of history. History with a purpose is worse than a novel with a purpose. Patriotism is a warm, emotional state, and a scientist needs a cool head for his work.

Lastly, scientific history must be wary of the use of literary devices. The historian's responsibility is primarily to the actors in his drama not to the watchers of his play; to the characters on the stage, not to the audience. He must keep in mind the facts he is dealing with, not be too much concerned with his method of presentation of them. I should like to use a quotation from Macaulay to show what I mean. In a brilliant comparison, in one of his historical essays, between the sixteenth and seventeenth centuries, he describes the Reformation as an occasion when "a great society rose up like one man, and emancipated itself by one mighty effort from the superstition of ages." Sonorous words and lofty thought are to be found in this sentence; but what about truth? What nation rose "as one man" in the sixteenth century to carry out the Reformation? Certainly not England with its deep and permanent division among Puritans, Anglicans and Catholics; not France with its Huguenot wars and uncertain outcome; not the Netherlands with its banishments and compromises; not Germany as a whole or any state of Germany. No historian would make such a statement if he were primarily interested in giving a truthful account; Macaulay was carried away by his habit of drawing literary contrasts or constructing balanced sentences. He forgot for the moment that he was an historian and became an orator or a stylist.

So scientific history must be non-partisan, secular, free from mysticism, engaged more with great movements than with great men, not warlike, not deliberately patriotic, not

too literary. Is the game worth the candle? Just for the sake of telling the truth are we willing to lose history as a pleasant field of literary production, a classified series of anecdotes, an effective method of propaganda, a useful armory from which men may select any weapon they find useful for their cause, whatever it may be? The case may not be so bad as that. So far as literary interest goes it certainly need not be that history should be dull. The life of man in the past is interesting in itself; it cannot be that it is dependent for its interest, when written about, on any false picture or romantic or sentimental light thrown upon it. The sharp clear lines of the novels of Thomas Hardy and Jane Austen, the portraits of Holbein, the guardsmen of Meissonier, the etchings of Pennell appeal to men, as well as more romantic story and more exuberant art. May the historian not count on a similar response to his "plain unvarnished tale" of real life? If history with all its varied materials acquired in these later times from so many sources, with its freedom from the disturbing duty of teaching religion, morals or patriotism, with its exemption from hero-worship, should simply tell the truth, the truth may not only be, accordingly to the old proverb, "stranger than fiction," but more interesting.

The phenomena with which the scientific historian deals are not merely chemical affinities and physical laws, "the architecture and mechanics of the atom," astronomical measurements and niceties of biological adjustment and evolution. If these can be made fascinating, not only to their professed students but to non-scientists, as has been shown at this meeting and elsewhere, how much more should the experience of our own kind, the life of man, with its tragedy and comedy and pathos, its personalities and races and customs and movements, serve as the material for a story that will thrill and charm its readers? The same qualities of mind that enable a man to write interesting but untrue history, would probably, if he had the training and made the effort, enable him to write interesting true history.

If history is dull the fault is probably in the man not in the method. In protesting against the preoccupation of historians with the literary side of their work I do not deprecate the production of good literature. But literary brilliance should be their secondary not their primary aim. Their first interest should and must be the truth of their narrative. The main question is that of motive, of intention. This must be to treat the phenomena of history as any other scientific student treats the material of his specialty. Only, as far as interest goes, the historian is more fortunate than they. "The proper study of mankind is man." Moreover interest may exist where brilliance is not called for. Much of historical writing, like much writing in the usually accepted fields of science, is for information, not exhilaration. Lucidity, simplicity, directness, vigor are all that can be demanded and all that are desirable in describing an historical institution, just as they characterize Darwin's statements of his observations of the habits of mating birds or of scenes on the voyage of the Beagle. Matters of unusual significance alone can be fitly described in terms of eloquence. A man who sees them as such will probably so state them.

Patriotism and morals as well as literary excellence may come by an indirect process. We love what we know. The region we have been brought up in looks the best to us. We stand by our family not because of the perfections of its members but in spite of their imperfections, and it may well be so of our fellow countrymen. A truthful narrative of the history of our country may be highly conducive to patriotism, even if it has to show that we were not always right in our wars and that our ancestors were men of like passions and imperfections with ourselves. It is probably as bad psychology as it is bad history to represent the early years of the nation as better than the later. A sense of reality of the past may awaken patriotism where an unreal story of visionary perfection may leave it slumbering.

Although it is no part of the duty of history to establish ethical principles, right and wrong may assert themselves in

the most dispassionate statement of historical occurrences. Mr. Lea says of history, "It may and generally will convey a moral—but that moral should educe itself from the facts . . . Righteous indignation should be a luxury left by the historian to his reader."

Therefore to assert that history should follow a scientific method is not to assert that it should not be interesting or patriotic or moral. May it not be that from a simple devotion to the ennobling task of writing true history will emerge a new eloquence and a greater persuasiveness to love of country and of goodness? "Seek ye first the kingdon . . . , and all these things shall be added unto you."

If history should once attain its true position as one of the sciences, a world of usefulness may open out before it analogous to the work of other branches of science. I am not unmindful of the full name of this society: The American Philosophical Society Held at Philadelphia for Promoting *Useful* Knowledge. In the first place, a scientific treatment of history may make it useful as a source of valuable experience applicable to modern problems. It has usually been worthless for such purposes because of the subjective and incongruous elements included in it. Secondly, a scientific study of history may discover laws analogous to those of physics or organic life, which will indicate lines along which we must act if we would act effectively. Again, history as a science may, perhaps even more than other sciences, because it is more engaged with the affairs of men, habituate men to conformity to its favorite standards, extreme care in testing sources of information, caution in inference, moderation in statement, tolerance in all judgments, and so lift the general level of intellectual and social life. But the promise of this paper has been already fulfilled. It was to show how history could be made scientific, to point out the road, not to describe the country to which the road leads. That is scientific work for many men for many years.

THE GROWTH OF ANGLO-AMERICAN LAW SINCE THE DAYS OF THE JUNTO

By HAMPTON L. CARSON

(*Read April 30, 1927*)

WITHIN the time allotted to me, I can do no more than sketch the outlines of so large a theme.

The most impressive feature of our judicial architecture —that which first strikes the eye and rivets the attention of the student by its simplicity of design and its noble proportions—is the dome-like jurisdiction of the Supreme Court of the United States. It fulfils the profound utterance of Edmund Burke that, "Whatever is supreme in a State ought to have, as much as possible, its judicial authority so constituted, as not only to depend upon it, but in some sort to balance it. It ought to give security to its justice against its power. It ought to make its jurisdiction as it were something exterior to the State."

The creation of that Court with its appellate powers was the crowning marvel of the lego-statesmanship of America. It is the finest and most precious conception of the Constitution. It embodies the loftiest ideas of moral and legal power, and although its prototype existed in the Supreme Courts established in the various States, yet the majestic proportions to which the structure has since been carried have become sublime. No product of government, either here or elsewhere, has ever approached it in grandeur. Within its appropriate sphere it is absolute in authority. From its mandates there is no appeal. Its decree is law. In dignity and moral influence it outranks all other judicial tribunals of the world. No Court of either ancient or modern times was ever invested with such high prerogatives. Its

jurisdiction extends over Sovereign States as well as over the humblest individual. It is armed with the right as well as the power to annul in effect the statutes of a State whenever they are directed against the civil rights, the contracts, the currency or the intercourse of the people. It restricts Executive as well as Congressional action to Constitutional bounds. Secure in the tenure of its Judges from the influences of politics, the purse, the army, the navy, the temptations of appointment to office or of patronage, and far removed from the violence of prejudice and passion, it presents an example of judicial independence unattainable in any of the States, and far beyond that of the highest Court in England.

Yet its powers are limited and strictly defined. Its decrees are not arbitrary, tyrannical or capricious, but are governed by the most scrupulous regard for the sanctity of law. It cannot encroach upon the reserved rights of the States or abridge the sacred privileges of local self-government. Its power is never exercised for the purpose of giving effect to the will of the judge, but always for the purpose of giving effect to the will of the legislature, under the Constitution, or in other words, to the will of the law. Its administration is a practical expression of the workings of our system of liberty according to law. Its members are the sworn ministers of the Constitution, and are the high priests of Justice. No institution of purely human contrivance presents so many features calculated to inspire both veneration and awe.

Speaking philosophically as well as historically the Court was not a creation but an evolution. In Franklin's day—the days of the Junto, which were the adolescent days of this Society—the thirteen colonies exercised the larger portion of their judicial authority through tribunals established directly or indirectly by themselves, under charters, grants, or proprietary forms of government, in accordance with the rules of the English Common Law, the principles of Equity, their own legislative Acts and usages, and such British Statutes as had been extended to or adopted by them.

But there were certain judicial controversies which were

not within the jurisdiction of the Colonial Courts; and there were others, also, where the jurisdiction so exercised was subject to an appellate authority in the Crown. This condition lasted during the entire period of Colonial dependency upon the throne. This period is illustrated by the Courts of Vice-Admiralty, established in some of the Colonies by the Crown, by virtue of a right expressly reserved in the charters, or, in some cases, without any such reservation. The Royal commissions gave to these Courts ample jurisdiction over all maritime contracts, and over torts and injuries as well in ports as upon the high seas. Under the Navigation Act of 12 Charles II. (c. 2)—which was copied from that of Cromwell tracing its ancestry through Tudor statutes to the reign of Richard II.—and under the Revenue Act of 7 and 8 William III. (c. 22), this jurisdiction was enlarged by giving or confirming cognizance of all seizures for violations of the revenue laws.

It was held that appeals lay from the Colonial Vice-Admiralty Courts, in all cases to the Admiralty in England. In questions of prize, an appeal lay to the Commissioners of Appeals, consisting chiefly of members of the Privy Council. Again, the Colonies had numerous disputes with regard to their charter boundaries, and these came before the King, sitting in Privy Council, who exercised original jurisdiction upon the principles of feudal sovereignty. Sometimes, the dispute reached the Lord Chancellor as in the celebrated case of Penn *vs.* Lord Baltimore over Mason and Dixon's line. Finally, a general superintending power by way of appeal was exercised by the King from the decisions of the local Colonial tribunals, in matters of private rather than public significance. Many instances of such appeals occurred in Pennsylvania during Franklin's day, and were common before the Revolution all through the colonies. In these phenomena we find the germs of the Appellate jurisdiction of the Supreme Court of The United States.

Passing from the Colonial to the Revolutionary period we enter upon a stage of struggling development. The

Continental Congress exercised, with the silent approbation of the Colonies, the underlying sovereign authority of the country so far as related to measures of war. After the Declaration of Independence, as a necessary consequence, the judicial authority of the Crown, as well as all its other authority, was suspended and ceased *ipso facto*. Congress raised armies and navies, directed military operations, emitted bills of credit, made treaties, received and sent out ambassadors, commissioned privateers, prescribed the objects of capture, and made rules for the distribution of prizes. It was in an urgent suggestion of Washington to the Continental Congress of the necessity of some central Court to deal with conflicting questions of prize that the germ of Federal authority is to be found.

In November, 1775, Congress recommended to the several Colonies that they should erect Courts of justice, or bestow jurisdiction upon those already in existence, to consider questions of capture, under such regulations as should seem expedient, reserving, however—and this is the important feature—in all cases an Appeal to Congress, or to such persons as might be designated by Congress, for the consideration of trials of appeals. That jurisdiction was first exercised by special committees appointed from time to time, called Congressional Committees of Appeals. Sixty-four cases—the records of which still exist—came before the Congress. Much difficulty, however, arose in the final enforcement of the authority of Congress, the weak point being that it depended upon State officers to carry into effect the judgment of the appellate tribunal when reversing a State Court. It frequently happened that the States refused to enforce the rights of property acquired under Federal decrees. It was not until after the Constitution of The United States had long gone into effect that the fires of controversy were extinguished.

In January, 1780, Congress grew tired of Special Committees, and established a Court for the trial of all appeals from the Admiralty Courts of the States, consisting of three

Judges commissioned and paid by Congress, under the style "The Court of Appeals in Cases of Capture." This tribunal took over eleven cases then depending before Congressional Committees and disposed of forty-five new cases, thus making in all a list of one hundred and ten cases disposed of by Federal Congressional authority. (See Appendix to 131 U. S. Rep.)

Controversies also arose between the States as to soil and jurisdiction. Pennsylvania and Virginia contended together as to their western boundaries. Pennsylvania and Connecticut contended over the Connecticut claim to the upper half of present Pennsylvania; Pennsylvania and New York contended over the "smoke stack" upon Lake Erie; New York, New Hampshire and Massachusetts disputed over the New Hampshire grants which now constitute Vermont; Virginia and New Jersey presented conflicting claims to that distant tract on the Ohio which is now called Indiana; New York and Connecticut wrangled over a boundary line fixed in 1651 by Dutch and English agreement in the days of Peter Stuyvesant of New Amsterdam, now the City of New York. Massachusetts and New York, and South Carolina and Georgia had their separate disputes.

Then came a third period—that of the attempted government of The United States by the Continental Congress under the Articles of Confederation. These Articles, assented to by Congress on the 15th of November, 1777, did not become operative until nine States had assented, so that action under them was delayed until March, 1781. The Ninth Article, drafted by John Dickinson and Benjamin Franklin, provided for four classes of cases, vesting in Congress the exclusive right and power to deal with piracies and felonies on the high seas, cases of capture, disputes between the States, and controversies between private citizens concerning the right of soil claimed under grants from two or more States.

The weak point was in the matter of enforcement. Reliance was had upon the judges of the Supreme Courts of the

States. State judges sat, as was natural, in a State court house, verdicts were sought from State jurors; prisoners, when convicted, were under the custody of State officers, and, if executed, were hanged by the State officer, the sheriff. Even infractions of the law of nations, of the immunities of foreign ambassadors, of the law of treason, and violations of the ordinances relating to the Post Office of The United States, were dealt with entirely through the State Courts.

Let me emphasize the point that under the Confederation no tribunal, State or *quasi* State, Congressional or *quasi* Congressional, ever pretended to be vested with the appellate authority, which, before the Revolution, had been exercised by the King in Privy Council from the decisions of the Colonial Courts. Appellate power is necessary to uniformity of decision, to effective regulation, to the enforcement of obedience. The Continental Congress, even under the Confederation, was not a government. It had a head and a body, but it had no hands. It could not take hold of a citizen or seize upon property. It was a loose league, for while the States were tied together after a fashion, individual citizens were not. The Congress could pledge the public faith, but could not redeem it. It could not raise any revenue, levy any tax, enforce any law, secure any right or regulate any trade. It could declare everything, but could do nothing. It was entirely at the mercy of thirteen State legislatures. It had no resource except persuasion. It dared not impose a fine, for it could not collect it. It could not imprison, for it could not act against the person of a citizen. Behind the Continental currency there was no taxing power. The only way in which money could be raised was by requisitions on the States, but compliance depended entirely on the good nature, resources and patriotism of the State Legislatures. Sometimes obedience was tardy; sometimes resistance was defiant. Every measure, however just, required the assent of nine States, and however urgent the necessity for immediate action, involved the fatal delay of debate in thirteen separate Legislatures.

From the deep pit of ruin, despair, civil paralysis, bankruptcy, disunion, discord and dishonor, into which we were cast after the Treaty of Peace acknowledging our Independence, we were saved by the Constitution of The United States. In the work of framing this instrument of salvation, Franklin, in spite of his weight of years, bore an honorable and effective part. The founder of the Junto—the father in a very real sense of this Society—he lived to become one of the Founders of the Nation.

I cannot enter upon a sketch of Constitutional Law, nor attempt a delineation of the manner in which the imprisoned power of the Constitution was released like steam upon an untried engine. The Appellate power of the Supreme Court of The United States was made effective by the Judiciary Act of September 24, 1789. Enough has been presented to give the tracings in outline of the origin and gradual evolution of that system of Appellate jurisdiction which has made the Supreme Court the guardian of our National rights and liberties—"an indivisible union of indestructible States."

As an old-fashioned lawyer I cannot but protest against recent efforts to graft upon the Constitution a bundle of police regulations foreign to its nature and destructive of the matchless symmetry of our system. Yet, I am optimistic enough to believe that the Constitution will survive all assaults, encouraged by the thought that the Pyramid of Cheops has stood four thousand years, unshaken by the barkings of the jackals at its base.

The second object of our attention consists of the group of State Constitutions. These, though of lesser dignity and scope than the Constitution of The United States, were of earlier origin, involving substantially the same principles of self-government. They were all evolved from the Colonial Charters which themselves were the product in modified form of the Charters of ancient towns, but particularly of great trading Corporations which sprang from the awakened commercial rivalries between nations following the discovery of the New World, and the seeking of shorter routes to the

Spice Islands of the far East, the gold coast of Guinea, the forests of Brazil and the mines of Peru. Corporate management under Charters, of which the Muscovy Company and The British East India Company were early examples, almost of necessity involved the ideas of representation of owners through boards of directors, business-like procedure, distribution of official powers, financial responsibilities, and ultimate accountability. They were all primary schools of what in time became ripened citizenship. The Selden Society, in a separate volume entitled Select Charters, has assembled nearly fifty examples which can be examined with advantage by the student of Anglo-American institutions before analyzing and comparing the Colonial Charters of the Old Thirteen.

The historical antecedents of these Charters belong to two distinct periods—the first being that of Discovery, stimulated by trade ambitions, extending from 1496 to 1606; the second being that of Colonial grants, animated in striking instances by the nobler motives of religious freedom and benevolent colonization, extending from 1606 to 1681, or, if Georgia be included, to 1732.

I have no space for exact analysis, but the rich material is easily at hand in the late Professor Thorpe's authoritative work, published by Congress, entitled Colonial Charters, and in MacDonald's Select Charters. Suffice it to say that all these Colonial Charters were carefully drafted by the most eminent lawyers of their day, while occupying the offices of Attorney General and Solicitor General of the Crown. Among these appear the famous names of Sir Edward Coke, Sir John Dodderidge, Sir Henry Hobart, Sir Francis Bacon, Sir Edwin Sandys, Sir Robert Heath, Sir William Noy, and Sir Creswell Levinz. Even Lord Chief Justice North, the Earl of Clarendon, and the philosopher, John Locke, had a hand.

As might be expected, the earliest charters placed legislative and executive powers in a Governor and Council nominated by the Crown and guided by its instructions. In

time, royal influence was checked by extemporized popular elements springing from colonial soil. Then, later, the burden of organizing governments was put upon the shoulders of single individuals, as proprietors, while in two instances the Charters constituted almost a pure democracy. With all this material the leading men of the day, especially Franklin, Dickinson and Dulany, were thoroughly familiar. Franklin had been especially disciplined by his long struggle with the sons of Penn; Dickinson of Pennsylvania and Dulany of Maryland by their legal studies in the Inns of Court, while the Adamses and Otis of Massachusetts were advocates of natural rights. Lesser men by their skill as pamphleteers displayed a complete familiarity with the successive features of the Bill of Rights of the days of James I., the Cromwellian Reforms and the results of the English Revolution obtained through the abdication of James II.

No one can examine the mass of politico-controversial literature of the period marked by the agitation over the Stamp Act, or study the State papers issued by the First Continental Congress, which evoked the open admiration of the Earl of Chatham, without being impressed with the knowledge displayed by their authors of the inherent rights of Englishmen under what was vaguely called the English Constitution, as asserted from time to time since the days of Magna Charta. Three works were especially in familiar favor—Hooker's "Ecclesiastical Polity," Hobbes' "Leviathan," and Milton's "Essay on the Liberty of Unlicensed Printing," in the matter of the development of far-reaching thought. Locke on "Civil Government," Montesquieu on the "Spirit of Laws," and Blackstone's "Commentaries" were of very direct influence in compressing political speculation into practical moulds. Locke embodied the essence of the English Revolution; Montesquieu insisted on a scientific division between Legislative, Executive and Judicial powers. Blackstone furnished the most finished and intelligible analysis of the English Constitution and gave "a new birth to the Common Law in America," through the happy circum-

stance of a Philadelphia publication of his work five years prior to the Declaration of Independence.

Hence it was, that when the Declaration, like a sharp knife, severed the tie that bound the Colonies to the throne, each Colony fell back upon its inherent Sovereignty, and at the suggestion of the Continental Congress, undertook to frame a Constitution for itself. In the distribution of the powers of Government between three Departments, Legislative, Executive and Judicial, the influence of Montesquieu is manifest. The Framers had been educated in the harsh school of experience to a practical application of the best political philosophy of their day. They were ready for their tasks.

Franklin participated very actively in the drafting of the first Constitution of Pennsylvania—that of 1776. It was never adopted by a popular vote, and is, as compared with later instruments, a crude piece of work. The same remark may be predicated of the earliest State constitutions adopted elsewhere.

We have as the result of the labors of the statesmanlike law givers of the period from 1775 to 1787, two distinct bodies of Constitutional literature which became the basis of that overshadowing Department of American law known as *Constitutional Law*—divided into *The Constitutional Law of the States*, and the *Constitutional Law of The United States*. In some respects they are similar, especially in the features of what may be termed Personal Rights or Liberties. In others totally dissimilar. The radical distinction is this: As each State was in itself a sovereign, the Legislative power of each State is absolute like the omnipotence of Parliament, save where expressly or by fair implication restrained by Constitutional restrictions. On the other hand, as the Nation was the creation of the People of The United States, legislative powers are but delegations of authority, and cannot exist except where expressly conferred, or can be fairly implied from express grants. Hence, National powers exist only where the warrant for their existence can be found in the

text of the Constitution of The United States, or is deducible by fair argument from express powers. State powers exist inherently except where expressly or impliedly denied by the State Constitution. No such distinctions can exist in England, as the British Constitution is not a written instrument. It follows that it is a unique feature of American Jurisprudence that every statute, whether State or National, is to be judicially tested according to the provisions prescribed in our written instruments of organic law.

It is true that the imposing edifices erected upon the foundations laid in Franklin's day have been extended to such vast proportions as to bewilder the contemplative observer, but the secret of their strength lies in the depth and breadth of view of men who knew how to avail themselves of the lessons and material achievements of the past. Successful governments have never been the products of pure theory. The most distinguished of truly beneficent reformers, while fiercely assailing entrenched evils, have never scorned to study human nature, or vain-gloriously proclaimed their schemes as essential to the happiness of mankind. They never concocted quack remedies or dealt in legislative panaceas.

All earnest students of our institutions know well that all public documents, whether charters, constitutions, legislative acts or judicial decisions are but expressive of political ventures, following but never preceding ripened public opinion. When embodying plans of government they have had a history. All grants, as well as limitations of power, if explored, can be traced to sources more or less remote. Such documents have respectable ancestors. However striking their characteristic features may be, they are never strictly singular, but bear the marks of their relationship. Moreover, if their objects be similar and they follow each other at appreciable intervals, they display in their variations an enlargement of ideas, and a progressive scale of thought. The biographies of those particular instruments which have played a leading part in the establishment or

development of our present institutions constitute an interesting as well as instructive chapter in the history of our legal growth since the days of the Junto in 1727.

The next structure commanding attention constitutes a vast department of substantive law, knowing no bounds of latitude or longitude but reaching, both in the making and in performance, the utmost ends of the globe. It embraces the multifarious activities of men in a distinctly commercial age and business life in all its aspects. It is comprehensively known as *The Law of Contracts*.

Its area is wider than any other department of the law, for apart from the preservation of the peace through the exercise of the police power, and the adjustment of private wrongs known as torts, the greater part of the work of government, as executed by Courts of Justice, consists in giving sanction to those engagements between men which rest upon that general expectation of good faith which prevails among all people of average right mindedness.

While the conception of Contract was of ancient origin, appearing in the Institutes of Manu, in the Bible, and in that great body of classical jurisprudence known as the Civil Law which Rome brought to the highest pitch of perfection in the fifth century, and which penetrated England in the days of Bracton in the thirteenth century, yet it was not until England became a commercial nation that the law of contracts emerged from a rude and crude condition. In *Ancient Law* Sir Henry Maine asserts that: "At first nothing is seen like the interposition of law to compel the performance of a promise." And the same learned author has elsewhere asserted that "the Society of our day is mainly distinguished from that of preceding generations by the largeness of the sphere which is occupied in it by Contract." The great forward movement in its awakening from the sluggish sleep of centuries, guided by the master mind of Lord Mansfield, began during the latter part of the life of Franklin, and has been since taken up and carried onward by American jurists at a constantly accelerating pace.

It will be understood that by the word *Contract* is meant *such an agreement as the law will enforce*, for it is well known that it is not every agreement that is enforceable, nor is it every promise that is binding. An agreement which is the result of force, or fear, or fraud, or mistake, or which springs from a mind diseased, or one enfeebled by age or infancy, or which has an immoral or illegal purpose, or is contrary to public policy, or in contravention of either constitutional or statutory law, will never receive the sanction of a Court of Justice.

With these limitations, an hour's reflection or a month's actual experience of life will satisfy any one that civilized Society could not exist without implicit confidence between men in their dealings with one another. When we say that a man's word is his bond we signify, in striking though not technically exact language, that legal as well as moral consequences flow from plighted faith between individuals, whether the pledge be oral or written as in the case of express contracts, or implied from acts or conduct which often speak louder than words.

To-day every act which does not involve a crime or a tort involves a contract.

Sir William Jones, as great as a lawyer as he was as an Orientalist, in his exquisite and scientific "Essay on Bailments," said: "There is hardly a man of any age or station who does not every week, and almost every day, contract obligations, or acquire the rights of a seller and buyer, or a hirer or letter to hire, of a borrower or a lender, of a depository or a person depositing, of a commissioner or an employer, of a receiver or a giver in pledge. Nor must it ever be forgotten that the contracts above mentioned are among the principal springs and wheels of civil society; that if a want of mutual confidence, or any other cause, were to weaken them, or obstruct their action, the whole machinery would instantly be disordered or broken to pieces."

To this impressive statement let me add that of Professor Sheldon Amos, in his work on "The Science of the Law":

The purpose of the law of contracts is to impart stability and security to certain temporary relationships with one another which men spontaneously frame for themselves. The relationship of two contractors differs from the relationship of family life in the spontaneity which originates it, and in the freedom which the parties enjoy for the purpose either of describing or modifying its terms, or of annulling it altogether. Thus the essential quality of the relationship implied in contract is freedom in respect of its original creation; in respect of the description of its nature and of its terms, and in respect of the mode and period of its conclusion. The real policy which dictates a law of contract is that of giving the same reality and consistency to the groups which evolve themselves through the play of social and economic life as primitive law gives to those groups, of which gradual formation is the indispensable condition precedent to the very existence of national life.

Paley, in his work on "Moral Philosophy," in a chapter headed with the direct and striking question—*Why am I Obliged to Keep my Word?*—analyzes the sources of an obligation to perform promises. He says:

Men act from expectation; expectation is, in most cases, determined by the assurances and engagements which we receive from others. If no dependence could be placed on these assurances, it would be impossible to know what judgment to form of many future events, or to regulate our conduct with respect to them. Confidence, therefore, in promises is essential to the intercourse of human life, because without it the greatest part of our conduct would proceed upon chance; but there could be no confidence in promises if men were not obliged to perform them. The obligation, therefore, to perform promises is essential to the same ends and in the same degree.

Dr. William Whewell, in his "Elements of Morality," gives expression to the same thoughts. He says:

We have already spoken of the necessity of mutual understanding and the consequent necessity of the fulfillment of promises as one of the principal bonds of Society. The necessity of dependence upon the assurance made by other men gives birth to the right in the person, to whom the assurances are made. A person has, under due conditions, a right to the fulfillment of a promise. The law realizes this right, and must therefore define the conditions. The mutual assurances which the law undertakes to enforce are called "Contracts."

Now compare this language with that of Sir Frederick Pollock in his "Principles of Contracts," and see how thoroughly the jurist agrees with the principles enunciated by the moralists:

"The law of contracts," says Sir Frederick, "may be designated as the endeavor of the State, a more or less imperfect one by the nature of the case, to establish a positive sanction for the expectation of good faith which has grown up in the mutual dealings of men of average rightmindedness. Accordingly, the most popular description of a contract that can be given is also the most exact one, viz.: that it is a promise or set of promises which the law will enforce. The specific mark of a contract is a creation of a right, not to a thing, but to another man's conduct in the future. He who has given the promise is bound to him who accepts it, not merely because he had already expressed a certain intention, but because he so expresses himself as to entitle the other party to rely on his acting in a certain way."

The principle is judicially stated by Lord Blackburn in Cornish *v.* Abington, 4 Hurlston & Gordon, 549:

If, whatever a man's intention may be, he so conducts himself that a reasonable man would believe that he was assenting to the terms proposed by the other party, and that other party, upon that belief enters into a contract with him, the man thus conducting himself would be equally bound as if he had intended to agree to the other party's terms.

Chief Justice Marshall, in Ogden *v.* Saunders, 12 Wheaton, 341, said:

A great mass of human transactions depend upon implied contracts, upon contracts which are not written, or which are not spoken, but which grow out of the acts of the parties. In such cases the parties are supposed to have made those stipulations which, as honest, just and fair men, they ought to have made.

Lord Esher, while Master of the Rolls, said, in Ex parte Ford, 16 Queen's Bench Division, 307:

Whenever circumstances arise, in the ordinary business of life, in which, if all persons were ordinarily honest and careful, one of them would make a promise to the other, it may be properly inferred

that both of them understood that such a promise was given and accepted.

It is clear then that the basis of judicial action rests on a fusion of ethical, utilitarian and practical considerations and that the legal answer to Paley's question, Why am I Obliged to Keep my Word? must be, because public and private interests demand it. These principles have been enforced by judges whose independence is the fruit of one thousand years of struggle, who, with very rare exceptions have faced the frowns of power, the influence of corporations, or the vengeance of the mob, "be that mob the upper ten thousand or lower." Chancellor Kent long ago remarked that "law reports were worthy of being studied even by scholars of taste and general literature, as being authentic memorials of the business and manners of the age in which they are composed. Law reports are dramatic in their plan and structure. They abound in pathetic incidents and displays of deep feeling. They are faithful records of those competitions, factions and debates of mankind that fill up the principal drama of human life."

Enough has been presented to illustrate the manner in which, since Franklin's day, English and American jurists have expounded and applied principles which underlie and control our daily actions. Let me now bring home to the minds of my lay hearers a realization of the extent to which they themselves have become involved within the last week. Those of you who have come from a distance, either to deliver or to listen to addresses during the celebration of this notable Bicentennial Anniversary of the formation of Franklin's Junto, had to rely on the punctuality and exact performance of hundreds of contracts, both express and implied. Apart from your personal contracts with the Railroad Companies embodied in your railroad tickets, you had to rely upon contracts to which you were not parties—upon contracts made between connecting roads for the conveyance of cars according to the time tables. The railroads relied upon the contracts made with car and locomotive builders, as well as

with the train crews—with the engineers, firemen and brakemen. They relied on the contracts made with track walkers, section men, telegraph operators, switch tenders, yard men and watchers in signal towers. All of them relied on the contracts made with miners for the supply of coal, and these in turn relied upon contracts for the supply of food, clothing and proper tools, as well as air supply and safety contrivances. It is true that performance in every case was guarded by the possible consequences of negligence in the performance of a duty, but the duties to be performed sprang out of contracts.

Let me extend the outlook. Credit is everywhere the corner stone of human enterprise. The man without credit is useless and helpless. He is not simply dishonored—he is a business paralytic. Multiply the individual whose word no man can trust by the sum total of the community, and the result would be chaos. Strike out of existence the single contract of Insurance, with its five-fold aspects of fire, marine, life, accident, and health policies, and cities would cease to be built, ships would disappear from the sea, and human life would be full of an agony of apprehension which would visit bodily mutilation, sickness and approaching death with terror. Strike down the faith in commercial promises which enables banks to discount paper, and business is at end. Strike down faith in corporate obligations of any form, and collaterals would shrivel into waste paper, and millions of men and women would become insane beggars. Undermine confidence in the undertakings of government, and national prosperity would vanish in the air. Extend these consequences to the limits of the globe, and civilization would be annihilated.

The curtain rises upon a vast scene, but we can spare time for but a single glance. *International Law*, like a distant sun, "swims into our ken." In Franklin's day the only writers of note—and they were then but little known—were Grotius, Puffendorf, Vattel and Burlamaqui. With the opening of the nineteenth century Sir William Scott, later and best known as Lord Stowell, was deciding questions of

maritime law, especially in cases of prize arising out of the Napoleonic wars; to be followed by Marshall, Story and Bushrod Washington in dealing with similar questions springing from our War of 1812. During and after our Civil War a similar service was rendered on a far broader scale by Taney, Chase, Waite, Miller, Bradley and Field, especially defining the meaning of the phrases *The Rights of Neutrals* and *Contraband of War*. Then came an impressive body of Jurisprudence, distinctly American, composed of judicial decisions, the text of treaties, the papers of our Secretaries of State, the opinions of Attorneys General of the United States, commented on and digested by Wheaton, Lawrence, and our distinguished fellow member, John Bassett Moore, and Charles L. Hyde. It is not too much to say in this presence that Judge Moore is everywhere regarded as the Grotius of the present.

I turn now to the *Law of Torts*, those private or civil wrongs which, though not amounting to crimes, involve injuries for which compensation must be made. They consist in breaches or violations of duty, which may spring from contracts or quasi contracts, or from such relationships as impose the obligation of the exercise of care not to injure one's neighbor in the manifold relations of life. It is largely of modern growth, and has reached vast proportions. The surgeon, the physician, the dentist, the lawyer, the nurse, the druggist, the manufacturer, the builder, the employer of labor, the common carrier, all public service corporations, telephone, telegraph, heat, light and power companies, the public press, the citizen driving his own car, and municipalities are all subjected to its sway. Professional men must possess and exercise that degree of knowledge, of judgment, of skill and of care which they profess to possess by holding themselves out as members of learned professions. The manufacturer and employer of labor must furnish to his workmen safe guards to machinery, proper tools, and reasonably safe and sanitary situations in which to labor: builders must underpin adjoining foundations, guard sidewalks from

falling materials, and be answerable to their workmen for the consequences of the negligence of co-employees. Common carriers must give their passengers safe conveyances and reasonable time in which to enter or leave vehicles before starting, and take precautions against collisions on rails or streets by proper brakes, and the observance of signals. Ships must load, stow and discharge cargoes with due care, observing both on the open sea and in port the rules of navigation. Newspapers must refrain from libels. Citizens must obey speed regulations, and respond in damages for assaults and batteries, deceit, slander, malicious prosecutions and conspiracies, and cities should not permit holes in the highways to menace travel. It is needless to multiply illustrations.

The doctrines of the law are covered by the general term *Negligence*, or an absence of due care. There is and can be no hard and fast rule. Every case must be judged of by its special circumstances. Negligence is either of commission or omission—the not doing of that which a man of ordinary prudence, under the circumstances, would attempt, or the omitting to do that which every man of average prudence would do under the circumstances.

If I may be pardoned an attempt to state it in briefest form the rule would be: To know what to do, and to do it; to know what not to do and to refrain.

In dealing with *prudence* there must be considered the advance of science, learning, art and mechanics as applicable to the particular locality in which its exercise is attempted. Circumstances must be considered. What would be clear negligence in one case, may not be avoidable in another. A well equipped hospital in a large city, for instance, differs from conditions prevailing in a small hamlet in testing a charge of malpractice against a surgeon, and the case of a motorman prevented by surprise from applying the brake in time to avoid running down a child darting suddenly from a narrow alley in front of his car, differs from the recklessness of uncontrolled speed in a crowded block.

The severity of the rule in exacting care is tempered by the doctrine of contributory negligence. The mere happening of an accident is not *per se* evidence that the injury was the result of the carelessness of the party sought to be charged. The injured person must be free from blame in having been equally at fault by inviting danger through his own negligence. "Stop, Look and Listen"—a pregnant phrase—is not confined to a railway crossing, but is applicable in principle to all situations in life. The closest attention to the facts, and the nicest considerations of the equities of cases are required of judges in charging juries or in controlling verdicts. These doctrines in Franklin's day were but little known, as the occasions for their application did not exist.

I now turn to *Crimes and their Punishment.* In no department of the Law have more remarkable changes taken place, during the past two hundred years. The most comprehensive division of crimes is into Felonies and Misdemeanors. Lord Coke, writing in the days of James I., named but eight felonies as existing at Common Law; that is by customary law, irrespective of statute, or by act of parliament. These crimes were treason, homicide (in its two forms of murder and manslaughter), rape, burglary, arson, robbery, theft and mayhem. Statutory felonies were so numerous that Lord Coke did not attempt to enumerate them.

Misdemeanors were crimes of less dark a dye, embracing all the lower grades of crime, from assaults and batteries to perjury and libel. But few of them existed by custom, and their number grew from century to century.

In addition to these there were many offences falling within one class or the other by special legislative enactment.

The punishment attached to all felonies, where the offender could not claim the benefit of clergy was death by hanging, forfeiture of lands and goods and corruption of blood or attainder. Benefit of clergy was an exemption from temporal punishment on the ground that the offender was in holy orders, of which the test in early days was the ability to read a text of Scripture. The benefit of clergy was not

permitted to treason nor to misdemeanors, and in the former the death punishment was added to by the sentence that the felon should be drawn and quartered. The phrase "felony without benefit of clergy" practically meant a crime punishable by death.

Punishment for misdemeanor was sometimes by death, more commonly by fine and imprisonment, sometimes by transportation for life or years.

Without delving too deeply into antiquity it is sufficient for my present purpose to state that in Saxon times human life was rarely forfeited as blood could be atoned for by money, and each man, according to his station, had his price. But as time went on, and benefit of clergy became established, the crime of murder went unpunished except by a branding in the thumb of the letter M for the purpose of identifying the recipient of the benefit, while the thief, who had no money, or who was illiterate, was hanged. This led to fraudulent impositions, by teaching a prisoner to recite a text of Scripture which he pretended to read from the Bible handed to him. Nor could the benefit be claimed by women, who were incapable of holy orders. Then came a gradual withdrawal of the benefit from the graver felonies. This was followed by the creation of new felonies without benefit. And so the death list grew.

It was by the creation of new felonies without benefit of clergy that the severity of the Criminal Law was greatly increased all through the eighteenth century.

In the second edition of the "Commentaries" published in 1769, Sir William Blackstone says that "among the variety of actions which men are daily liable to commit no less than 160 have been declared by Act of Parliament to be felonies without benefit of clergy."

The true explanation of this astonishing number of crimes existing in the days of Franklin as contrasted with the fewness in number in former years is not because our ancestors led purer or better lives in past centuries than in the times of Franklin. The phenomenon is explained by the circumstance

that one generation never repealed the laws of their predecessors, and so the mass went rolling on from century to century, augmenting in bulk, black with terror, heart-sickening in its atrocities.

The bigotries of Kings, the avarice of Queens, the ambitious plans of the Nobles, the fanaticisms of the Clergy, the selfish pleasures of the rich, the jealousies of land-owners, the greed of jailers, and the interest of scheming monopolists alike demanded victims, and cried out for blood.

The following are some of the offences which English Kings and Queens, Lords and Commons thought worthy of the death penalty: Maliciously to burn stacks of corn, or kill cattle in the night; to personate bail; to counterfeit lottery-tickets, stamps, exchequer bills; to blanch copper and mix it with silver; wilfully to destroy any ship; to assault a privy counsellor in the execution of his office; to steal a pump from any ship; to burn any wood or coppice; to return from transportation; to take a reward for the recovery of stolen goods; to tear, spoil or burn the garments of any person in the street or to engage in smuggling.

The Black Act in the year 1736, which was some nine years after the formation of the Junto, added to the list: To appear in disguise in any forest, path or heath, and wound or kill deer; to rob any warren of rabbits; to steal fish out of any river or pond; to cut down a tree in any avenue or orchard; to set fire to any house or barn—these acts were punished by death; to send threatening letters, anonymous or signed by fictitious names, demanding money, venison or other valuable thing; to break down the ledge or mound of any fish pond, whereby the fish might escape; to damage Westminster Bridge; to enlist in the service of any foreign prince without his Majesty's leave; to steal sheep or cattle; to steal linen, fustian and cotton goods and wares from bleaching fields; to cut or destroy velvet in the loom, or to destroy any tools used in the weaving of velvet; to forge any debenture bond relating to the duties of excise upon beers, cider and hides; and the draw backs on wines and ales;

to steal from the person to the value of five shillings—all these were punished by the dreadful penalty of death.

Yet, this was the England of Hale, of Addison and Pope, of Burke and Wilberforce, and Hannah More!

In the year 1810, Lord Holland in a Debate in the House of Lords, declared that no less than 234 offences were punishable by death.

I have not had the opportunity of verifying this count.

In 1763, frequent mention is made, in the books, magazines and newspapers, of bodies of malefactors, conveyed after execution to Black Heath, Finchley and Kennington Commons or Hounslow Heath, for the purpose of being there permanently suspended.

"In those days," says Charles Knight in his "History of London," "the approach to London on all sides seems to have lain through serried files of gibbets, growing closer and more thronged as the distance from the city diminished, till they and their occupants arranged themselves in rows of ghastly and grinning sentinels along both sides of the principal avenues. And, by way of a high temple of the gallows, in a central point toward which all these ranges might be supposed to converge, stood Temple Bar with its range of grinning skulls, beneath which, when the gory heads were first stuck up, Horace Walpole saw the industrious idle of the city lounging with ample store of spy-glasses, through which passengers were allowed to peep at them for the small charge of one-half-penny."

Whole counties were covered by the gibbeted remains of human beings, the air was poisoned by the stench of rotting limbs, and belated travelers, hurrying over the heaths, were startled at each lonely cross-road by swaying bodies hung in chains and glistening in the moonlight with ghastly horrors.

In the year 1816, at the very time when Sir Samuel Romilly was pleading for the repeal of sanguinary laws, there was a child in Newgate prison, not ten years of age, under sentence of death for stealing an article in excess of the value of one shilling. The Recorder of London was

reported to have said that "it was intended to enforce the law strictly in the future, to interpose some check, if possible, to the increase of youthful depravity."

English conservatism was strangely stubborn, and resisted for years all movements of reform. The scalding tears and flowing blood of thousands of unhappy victims had failed to thaw the frozen heart of justice. I have not the space within the limits of this paper to trace the history of the amelioration of the criminal code. Humanity finally triumphed; the self-sacrificing labors of Howard, the caustic wit of Sydney Smith, the eloquence of Brougham, the white-souled philanthropy of Romilly, and of Sir James Scarlet, later Lord Abinger, acting on the quickened intelligence and sensitive conscience of the nineteenth century, prevailed at last, and this barbarous code, with all its attendant horrors, passed into history, fit only to be compared to that regulation of the Inquisition which denied to suspected heretics the assistance of the law.

This result was attained but slowly—the advances being made chiefly during the reigns of George IV., William IV., and Victoria. It was not until 1861, by a general consolidation of criminal Statutes, that punishment by death was confined to treason, murder, piracy with violence and setting fire to dock-yards and arsenals. The curious student can trace the steps through the History of the Criminal Law of England by Sir James Fitzjames Stephen.

In Pennsylvania a far milder condition prevailed. This was due to the resolutely benevolent humanity of Penn, and also to the circumstance that life was free from those complexities which legislative bodies are prone to consider as calling for correction and restraint by penal statutes. Penn was the noblest, the boldest and the most immediately successful law reformer of the Anglo-Saxon race. He accomplished, at the outset, what it took England two hundred years to obtain. He was favored by having a virgin territory in which to try out his Holy Experiment. He encountered no entrenched interests, and, under his charter, was invested

with ample powers. He had, it is true, to have the approval of the freemen of the Province in securing laws, but he had sagaciously agreed in England with his first purchasers upon a series of laws, which were enlarged and ratified upon his arrival at Chester. At a single stroke of his pen he struck off all the saguinary punishments of his parent country respecting felonies, substituting, in lieu of death—save as to the one crime of murder in the first degree—temperate imprisonment and hard labor, and declared that "all prisons shall be work houses." He was unable, however, to hold his ground. His prolonged absences from the Province, the opposition of the Privy Council to many of his regulations, the veto by the Crown of many of his statutes, the appearance of turbulent characters, the outgrowth of interests conflicting with the public peace thwarted his intentions. He was too far in advance of his time, and marked recessions took place.

Pirates appeared in the Delaware, smugglers violated the revenue laws, forgers and clippers of the coin were active; burglars, incendiaries, ravishers, and thieves, no longer dreading death, plied their nefarious trades. And so it came about that under the Act of 31st May, 1718—the very year in which Penn died—all through the days of Franklin, the death penalty was attached to high treason, including all those treasons which respect the coin; petit treason, murder, robbery, rape, unnatural offences, malicious maiming, manslaughter, witchcraft and conjuration, arson, counterfeiting, and uttering counterfeit gold and silver coins and bills of credit, piracy, and concealing the death of a bastard. All these crimes, except *perhaps* the impossible one of witchcraft, were capital at the Revolution. Various efforts were made to ameliorate this code, particularly in 1786 and in 1790, but the most effective blow to cruelty was by the Act of 22d April, 1794, which, after abolishing the benefit of clergy, succinctly declared that "no crime whatsoever, hereafter committed (except murder of the first degree) shall be punished with death." The act then proceeded to define murder of the first and second degrees, and to proportion

punishments by imprisonment to the varying character of numerous offences. (See 1 Smith's "Laws of Penna.," 105 *et seq.;* 3 Smith's Laws, 187 *et seq.*)

It is a matter of pride to us to know that this truly great statute, which, by its comprehensive provisions, anticipated the aggregated results of all of the criminal law reforms of England by more than sixty years, was the work of William Bradford of Pennsylvania, one of our fellow members, who, inspired by the benevolence of Penn, sustained his arguments by a close study of Montesquieu and Beccaria. In a Memoir presented by Bradford, by request, to the Governor of Pennsylvania, published in 1793, he laid down principles which have become axiomatic: "that the prevention of crimes is the sole end of punishment;" "that every punishment which is not absolutely necessary for that purpose is a cruel and tyrannical act;" and "that every penalty should be proportional to the offence."

In 1795, the Memoir was published in London, and had a profound effect. It was hailed as "the production of a man of taste, genius and reading," that "every expanded mind that deplored the unnecessary destruction of human life by public executions" would "derive exalted satisfaction" from its perusal.

That which chiefly troubled Franklin was the transportation to the colonies under judicial sentences of British convicts, whom he regarded as undesirable citizens. He dealt with the evil in his characteristic way. Instead of addressing a direct protest to the Prime Minister, he sent a large box filled with live rattlesnakes to Sir Robert Walpole, with a letter assuring him that if the serpents were let loose in Kew Gardens they would prove to be no more harmful than the "rattle-snake convicts" shipped to America. When the Declaration of Independence put a stop to the practice, the British judges resorted to Australia as a dumping-ground for those who escaped the gallows through the humaner sentence of transportation.

The Constitution of the United States, and the State

Constitutions by forbidding the imposition of "cruel and unusual punishments," by the gift of the rights to a defence by counsel, to copies of the indictment, to the compulsory attendance of witnesses and to a trial by jury, marked the American advance in criminal law reform at least fifty years before most of these rights became established in England.

In 1827, and again in 1860, Commissioners were appointed to revise the Penal Code of Pennsylvania, and the results of elaborate reports were enacted into Statutes, which covered the management of prisons. At present, the subject is receiving the careful study of judges and members of the bar.

I must bring this rapid sketch of great movements to a close. I have been obliged to confine myself to the vital organs of the matter, without dwelling upon functions or mechanisms. I have necessarily omitted all reference to Private law—the law of Personal Relations, of Husband and Wife, of Married Women, of Parent and Child, of Guardian and Ward; of Trustee and Cestui que trust; of Title to Land, of Insurance, of Bonds, Mortgages and Liens, of Commercial Paper, of Partnership, of Corporations, of Banks and Banking, of Railroads and Common Carriers, of Sales and Bailments, of Receiverships, of Combinations in Restraint of Trade, of Evidence, of Equity Practice, of the Abolition of Imprisonment for Debt, and Improvements in Procedure. There are many things yet to be done, but they are on the way.

Sufficient has been submitted, I respectfully submit, to convince the most skeptical that Law, the most conservative of the Sciences, is not static, but has advanced as rapidly as was consistent with safety in the progress of the years.

GEOGRAPHY AMONG THE EARTH SCIENCES

By ALBRECHT PENCK

(*Read April 30, 1927*)

All systematic knowledge of the earth begins with the observation on the surface of the earth. The science of this surface—*geography*—is, historically speaking, the foundation of all sciences concerned with the earth. Its name was coined by Erathostenes, the same man who was the first to attempt the measurement of the globe, the spheric form of which had been revealed by observations of the firmament. But the study of geography dates back much farther, and like the surface of the earth, the atmosphere came in for early attention. Four books were devoted by Aristotle to *meteorology*. All the other sciences concerned with the earth were developed only in modern times. The eighteenth century brought the beginnings of *geology*, when the experiences gained in mining and through the exploration of the high mountain ranges were utilized in examining the crust of the globe. Even before that, *geodesy*, the science determining the figure of the whole earth, had developed. It is rooted in the activity of the ancient surveyors—the geometers—on whom both the abstract science of geometry and concrete geodesy are based. The latter name, though, is a reminiscence of the "field-dividers" who used to restore obliterated field boundaries. Finally, in the nineteenth century, there came the beginnings of the systematic application of natural science for the purpose of solving problems concerning the earth. The *geophysical* science includes meteorology. At present, the two are on the way to be separated, the former being more particularly concerned with the interior of the earth, which cannot be approached by the searching geologist and can be known only by the application of physical methods.

Besides, forming a parallel science to meteorology, *hydrography*, the science of the waters, with *oceanography* as its main part, has been strongly developed and *glaciology* too assumed the character of a distinct science.

Seven sciences are now participating in the investigation of the earth. Each of them has its definite aim and its specific sphere of activity. *Geophysics*, in the narrowest sense of the term, has to deal with the unapproachable and invisible interior of the earth, the quality and condition of which can only be inferred from physical effects at a distance. It receives important suggestions by the comparative observation of other planets. The tangible and visible crust of the globe is the province of *geology*. The latter derives its foundations from the nature and stratifical position of the rocks. It employs chemical and mineralogical methods, as well as field observations, to reveal the age of the rocks. It is supported by the study of the remains of organisms imbedded in the geological strata. Their treatment stamps it in many respects as an historical science, but the history of the earth thus gained does not even nearly exhaust the whole extent of geology as the doctrine of the crust of the earth. *Geodesy*, like every other measuring science, keeps in very close touch with mathematics. Like geography, it sticks to the surface of the globe, but it is unable to understand the latter without a constant appreciation of the interior. Accordingly, it has developed close relations to geophysics. *Meteorology*, *hydrography* and *glaciology* are prominently using physical methods of research and are therefore usually classified with geophysics. But the atmosphere, the hydrosphere and the ice are not so unapproachable as the interior parts of the earth. Their condition is not merely inferred from effects at a distance, but is determined by direct observation, and it is possible to penetrate deeply into both. So we class these sciences to be treated in the same category as *geology*. In the course of the last two centuries *geography* has yielded so much ground to its daughter sciences that to some it no longer appears as

an independent science. Yet, there is still a wide field of activity left for geography, even when confining it to the most narrow limits of its original contents, viz., to the surface of the globe. This surface is of special importance to the whole body of the earth, not as a boundary against the surrounding space—that we are looking for at the upper limit of the atmosphere—but because on it solar energy is converted into a number of different phenomena. To the study of the forms of the earth's surface, therefore, is added the research of the processes taking place on it, in other words, the morphological observations are supplemented by physiological ones. The latter, however, not only comprise the phenomena of inorganic nature, but deals also with those of the organic world. All organic life on earth is tied to the earth's surface, and therefore the latter is the actual scene of life, the Lebensraum; on that scene man lives and acts. Much closer, therefore, than the relations of geology, are those of geography to biology. And in geography, too, man plays an active part.

The sciences concerned with the earth are divided and classified according to different fields of research, not according to the methods employed by them. Quite naturally, each of them uses such methods as are especially applicable to its particular field, but not one of them confines itself to methods peculiarly its own. Not only are geophysics, in the broader sense of the term, dependent on purely physical methods, but in more recent times these are also frequently used by the geologist for the purpose of discovering the composition of the parts of the crust which he cannot reach; and to geodesy in its modern development they have added great scientific depth. The method followed in geological field observation looks original, but the very same process of separating the older from what is of more recent origin, and of marking certain horizons by findings peculiar to them, is used also in archæological research. Undoubtedly, geography more than the other sciences investigating the earth, has to deal with the geographical distribution of individual phenomena, yet the use of such a "geographical" method is

not a peculiarity exclusively its own. Both the zoölogist and the botanist must give attention to the distribution of animals and plants; and on the other hand, not everything connected with the distribution of phenomena pertains to geography. Measuring methods are not exclusively applied by geodesy, but by every other science as well where dimensions have to be exactly determined. This is particularly true of meteorology, hydrography and glaciology. The geophysicist can no more get along without mathematics than the geodesist, and the more exactly the geographical and geological sciences are treated, the more closely will they get in touch with mathematics. Quite frequently geology and geography have been contrasted with each other as the sciences of time and space, respectively. Certainly geology is strongly tinged with history, but tectonic geology cannot do without the chorological observation which has so often been taken as a geographical peculiarity. And geography in its turn, whenever dealing with processes, cannot omit proper consideration of the time for which they were going on. What divides the sciences concerned with the earth is not the methods to be employed but the objects to be investigated.

These objects, however, are not strictly separated from one another, but they are merely individual parts of a large whole. Sharp lines of division do not exist between the sciences regarding the earth any more than they exist, say, between dermatology, histology, and anatomy. Not even is the surface of the earth separated from its body like a skin; it is merely a surface, varied both in form and in the effects it produces. Rather, the crust of the earth might be compared with a corporal skin; but in any event it is not clearly set off from the interior. The working spheres of geology and geophysics intermingle just as much as those of geography and geology. There is, indeed, a good division between the respective spheres of meteorology and hydrography, yet the way of the water from the sea to the air and the land produces most intimate relations between them. How close a connection there is between geodesy, geology, geophysics, and

geography shall be shown by an illustration. Neither through their methods of research nor with respect to their contents can the various sciences of the earth be sharply distinguished from one another, and instead of the indistinct lines of division, the close touch between them ought to be pointed to. None of them can exist without the others. They all fertilize one another. The dividing principle between them is the proper division of labor which should by no means stop at their respective boundaries, but should see that those boundaries are not unduly emphasized.

What the geographer considers as the surface of the earth is partly the uneven surface of the rigid crust of the earth including glaciers, and partly the surface of the running and standing waters. Underlying are the litho- and the hydrosphere, above is the atmosphere. On it all the changes of energy are going on that are watched by the geographer. Here is the base of all life which rises but slightly into the atmosphere, which does not penetrate deeply into the ground, and which in the ocean is richest near its surface. This *geographical* surface of the earth is represented on our maps. Its uneven parts are looked upon as deviations from an ideal figure of the earth which is regarded as a surface curved on all sides. This surface is projected on the plane of the map. The geodetic investigations of the eighteenth century have shown that the flattened spheroid corresponds to that ideal figure of the earth; on it all maps and areas of countries are based by the geographer. In the nineteenth century, however, geodetic science has proved that the spheroid is no more than an approximation, though a very close one, to the surface which omitting waves and tides, exists in the sea level. This is a surface of a peculiar type and cannot be defined mathematically. While we regard all elevations of the earth as divergences from the normal level of the sea, still we do not base our maps on that level but on the surface of the spheroid, which is a close approximation to it; the astronomical coördinates are corrected by the amount of deviation from the plumb-line and then used in

our maps as geodetic coördinates. The great advantage of this procedure lies in the fact that, with proper selection of the projection, the distances between neighboring places may be correctly laid down. These distances can differ by 1/40 from the distances corresponding to the astronomic positions of the places. The latter which had originally been laid down in the maps, are not longer shown in our modern special maps. The whole complex of geographical mapping is now dominated by geodetic views.

The deviations from the vertical, as is well known, are due to the fact that the plumb-line at any place is not exclusively determined by the gravity of the whole earth, and by the centrifugal force, but is materially influenced by the irregular distribution of masses on the surface, and especially by the change from high to low. Every mountain causes a deviation of the plumb-line, but with very high mountain ranges it fails to correspond to their elevation. It is plain that the excess mass of elevations is counteracted and practically balanced by deficiencies in the depths. Below the continents there are portions of the crust that are too light, and below the oceans such that are too heavy. The crust of the earth is kept in a state of balance similar to the hydrostatic one. Dutton has called this fact isostasy. Thus geology derives one of its most important results on the crust from geodesy.

The isostatic balance of the crust of the earth is not perfect. While there are areas where it is very well developed, in others there prevails no more than an approximate balance. There are different theories explaining how it is produced. According to Pratt, above a balanced area in the depths there are different high posts whose density is inversely proportional to their height. According to Airy, "floes" of different thickness and equal density are floating on a liquid support of greater density. Geodetic investigations have determined the depth of the level of compensation at about 100 km., and are in harmony with Pratt's view. A grand experiment made by Nature, however, speaks for Airy's view.

During the glacial epoch, vast masses of ice were deposited on the land, considerably disturbing the isostatic balance. Northern Europe and North America were pressed down by their weight and are now swelling up again in a gradual rise. It may be conceived that the support underlying the ice was compressed and is now elastically extending again. But the investigations made by Daly in North America, and those simultaneously made by myself in Europe, have shown that during the ice-age not only the land supporting the ice cakes subsided, but the surrounding portions were pressed upwards, like tough loam around the foot sinking into it. The liquidity of those masses, though, is by no means great. In ten thousand years, after the ice had melted away, the land which supported it has not yet risen to its original elevation, nor have the masses pressed out from under it resumed their original positions. Airy designated the masses underlying the rigid crust as lava. We will call them magma.

Changes going on on the rigid crust must be followed by movements of the magma if the isostatic balance is to be maintained. But the great majority of all crust movements do not appear to be a warping due to the deposition or removal of heavy weights. Rather the reverse is the rule: Rising masses are lightened, sinking ones are pressed down, that is what we observe in most epirogenetic movements. The causes for them cannot be within the crust, but must be below it, in the magma. It is conceivable that the latter is swelling up, thereby raising its covering, or that it is contracting and accordingly its covering is lowered. But it is also conceivable that it moves laterally. That such lateral currents take place in the magma is inferred from the great geosynclinals. The latter are parts of the crust which at first keep sinking during entire geological periods, which cannot be understood unless masses below them are constantly flowing away. Next, the vast deposits are generally pushed together laterally. Yet they do not swell up in corresponding measure; instead, as Ampferer aptly remarks, they are swallowed up in the depths. Where, as for instance

in Finland, such geosynclinals are worn down very deep, the sediments drawn down to the greatest depths can be seen to be penetrated with volcanic rocks—they have been dipped right into the magma. At other spots of the surface of the earth large masses of magma have been poured forth or injected in the crust. Such places where the magma has evidently been very active, are places of confluence of magma. The rather complicated mechanics of the crust movements appears to us merely as a concomitant phenomenon of magma movements going on in the depths and carrying along suspended pieces of crust, in the same manner as moving water carries floes. These floes are here pressing and jarring against one another and are there migrating from one another.

At any rate, there is a great deal of motion in the crust of the earth, not only vertically but in the horizontal plane as well. It is easier, however, to recognize vertical movements, especially at the sea level, than those going on horizontally. The fact that North America is moving away from Europe has no more been proved by astronomical methods than a movement of Greenland. But during earthquakes there are also disruptions in the horizontal plane. The quake at San Francisco in 1906 furnished much occasion for thought in that respect. It looks as if North America had turned some distance along the edge of the Pacific. Alterations in longitudes, latitudes, and elevation took place suddenly on that occasion, reminding us that the geographical coördinates of a place cannot be regarded as fixed values. Of course, such alterations should not be accepted as proved, merely because a more recent determination of geographical positions resulted in different values from those found before. It should always be kept in mind that a second of the arc measures 31 m. on the surface of the globe and therefore topographic determinations have to be reliable up to fractional seconds if they are to show dislocations of a few meters. Few of them will be found so accurate, especially those of older dates. Therefore the fact that a more recent spirit-leveling yields different values from those of an older one is not

always a sufficient evidence that there has been a change of elevation. It was a mistake to have inferred such a change from the differences shown between two level surveys of France. Proper consideration must always be given to the unavoidable errors of measurement, and especially to the question whether equal care has been taken in each instance in eliminating them. In connection with geodetic operations, however, the variability of the geographic coördinates of the fixed points should be kept in mind. Possibly the difficulties often experienced in reconciling trigonometrical surveys of different age of measurements with one another are connected with dislocations of the triangular points which have occurred in the intervals between the dates of measurement.

Geological investigations make it entirely sure that frequent shiftings of portions of the earth's crust have occurred toward one another. Every crumpling evidences them, every genuine folding points to compression, every normal faulting to expansion. The frequent occurrence of folds has helped in advancing the view that the crust of the earth was "crumpled" over a shrinking nucleus. But frequently spots are met with where wide gaps have been closed by magma entering into them. It has not been surely established that the crust has grown smaller in the course of geological history. That in spite of all the numerous shiftings in connection with earthquakes, and in spite of all volcanic eruptions, and of all additions by meteors, its size has not changed in historical times is taught by the constant length of the day. We are therefore talking only of the shifting of points on the surface of the earth in relation to each other, and again there appears before our mind's eye that picture of floes, jarring and pressing and pushing up their edges upon one another, while at other spots open water appears between them. This view is not identical with that of A. Wegener, according to whom the continents, built up of salic masses, are floating like icebergs on the fluid sima. For, the bottom of the sea is not fluid but rigid like any other portion of the crust. The low temperature at the bottom of the sea renders it

impossible to conceive that a molten sima could be near it. Rather it indicates that the stiffening process of the crust must have penetrated deeper below the ocean than below the dry land. If Wegener's idea that the continents are salic floes dipping into the sima of the ocean's bottom, is correct, then they got thoroughly frozen in there and cannot have a considerable movement of their own.

Of course, in talking not only of a certain degree of mobility but even of actual movements of the magma below the crust, we are far from asserting, as has formerly often been done, that the interior of the earth is fluid, "molten," as it is usually termed. A high degree of rigidity of the earth is clearly indicated by the investigations about the tides. At the same time, its present figure conforms to its rate of rotation, and no greater flattening at the poles is left as a reminiscence of the time of a presumably higher rate of revolution. The body of the earth yields to forces which are steadily exerted. It is not strictly rigid but viscous, and the solution of the problem seems to be that the interior, which is not homogeneous materially, is no more so with respect to its viscosity. The latter is probably least in the central parts, increasing towards the exterior and being greatest right below the crust.

That the surface of the globe is steadily changing has been recognized already by the ancients, and the fact has ever since received attention. A century ago, K. A. E. von Hoff collected the many accounts existing about natural changes of the earth's surface, and from them came to the conclusion that "all the alterations on the surface of the earth, observable to us or actually observed by us or inferred from correct observations of nature, have been exclusively caused in the course of great, in fact, very great periods of time by the action of those natural forces empirically known to us, such action, while gradual on the whole, still being now and then and here and there more rapid and pronounced." He banished catastrophism from geology and placed the latter on the correct basis of an empirical science which does

not rely on what is unknown but instead adduces known facts for comparison in interpreting its observations. Like Lyell, he provided geology with the foundation derived from geographical observations, gathered in the rich store of tradition, and thoroughly imbued with the view of Aristotle who says: "Since, however, all natural evolution with respect to the earth proceeds gradually and in the course of very long periods of time as compared with our lives, such evolution remains unnoticed." Lyell, on the other hand, built on his own observations and drew the attention of the geologists to the natural alterations of the surface of the earth, which have ever since been examined by them almost more closely than by the geographers. In doing this, however, he paid more attention to the gradual sedimentation than the wearing down processes going on at the same time.

In one respect von Hoff and Lyell especially agree, viz., in their demand for long geological times, extremely great periods. These were inferred from the great thickness of the various deposits, the calculations being based on average values of the rate of speed at which they had been formed. No wonder that the results were widely different from one another and the estimates of one author showed the influence of the Bible, while those of another betrayed more of the Artistotelic spirit. William Thomson, pointing to the loss of heat suffered by the earth and sun, cut down the geological periods more than was consistent even with moderate estimates. Now investigations made about the decomposition of uranium provided the geologists with periods of the needed size and put an end to the guessing and groping and confirmed those geological conclusions which were made on a geographical basis.

The Aristotelic idea of a gradual and steady transformation of the crust of the globe is now the view controlling all research. The catastrophes have been relegated to the arsenals of laymen's views. A great part of the lithosphere consists of sediments of the same kind as are formed even now. Each of the strata is a piece of a former crust surface

which was buried and thereby conserved, usually more or less deformed by the movements of the crust. Other rocks have come up from the depth; they are either poured out on the surface or injected between other rocks, having assumed different structures on the way to their cooling. They testify to the great mobility of the magma. It is recognized that some of the rocks were dipped into the latter and thereby changed and even melted in. Thus the crust of the earth is made up by a whole series of stratified earth surfaces and of the effects of magma. It appears as a set of earth surfaces and magma kneaded together. The older view that in the course of geological history the earth had gradually cooled is untenable: Both at the end and at the beginning of geological history there is an ice-age, and quite enigmatically there enters into it, during the permocarbonic period, a time of widespread glaciation especially in the lower latitudes of the globe. Times during which what is now dry land was widely flooded alternated with others when the land covered a wider area than now. Some recognize a certain rhythm in the crust movements, a succession of alternating evolutions and revolutions; but our geological knowledge of the surface of the earth is not so extensive, and the determination of palæontological times is not sufficiently certain, to exclude the assumption that differences of intensity of crust movements as are noticeable are due more to changes which happened in space than to those which succeeded each other in time.

In what direction the evolution of the earth will proceed hereafter cannot be foretold. In fact, we do not even clearly know its direction in the past, and on that account it has been denied that the earth is aging. Ignoring the unknown goal and viewing only what is going on and is proved to have gone on in the past, there remains the principle that terrestrial history is characterized by gradual evolution. The *evolutional view*, which has for 80 years been the controlling principle of biology, originated in geology. Darwin stands on the shoulders of Lyell. Geological history has not only

furnished the finest proofs for the gradual evolution of the organic world, but has also demonstrated that this evolution leads up from the simpler to the more complicated forms. The question, however, whether the struggle for existence is the great promoter of this progressive development, may be left undecided. The last of the great events in geological history, which ought to have vastly developed it, has been of rather insignificant effect on the organic world. During the last glacial period millions of square miles have repeatedly been buried under glaciers; from these areas the ice pushed out all forms of life, and they have since returned; at times the organic world was compressed, at others it could expand again, yet there has been but a very slight change of forms; old ones disappeared, but only few new ones made their appearance. It is true, however, that all this was going on within a short period of time, in fact, within a few ten-thousandths of the span of geological time from the pre-cambrian to the present time.

The victory of the evolutional view in the sciences of the earth and of life is the grand achievement of the nineteenth century. This view is based on the application of a comparative method which in many instances recognizes the effects of an historical succession in what is coëxistent in time. Feeble starts toward such a comparative method may be traced in Karl Ritter's conception of a general comparative geography. Lyell developed it by explaining geological phenomena through the observation of processes going on on the surface of the earth, *i.e.*, geographical processes. More than is generally known, has geography influenced the development of modern science, and if we are looking for the mutual relations existing between the various sciences concerning the earth, we find geography to be the connecting link. For it is only on the surface of the earth that all the observations regarding its crust and the depths of its interior can be made. They are all dependent on geographical work, since most observations are worthless without a knowledge of the situation of the points where they are made.

This central position of geography among the sciences concerned with the earth is rarely appreciated. The reason is that we frequently fail to realize what we owe to it, and in most cases the required geographical information can be easily procured from atlases. And because it is not realized what an amount of geographical research is contained in our atlases, it may happen occasionally that geography is denied the quality of a science. The knowledge of the terrestrial surface must not necessarily remain superficial. As a matter of fact, it is true that the geographical surface, both of the continents and the oceans, can be dealt with by other sciences as well. Geologists cannot be prohibited from giving attention to the surface of that crust which they are investigating, and to the geodesist the surface of the sea, freed from waves and tides, is the visible geoid.

But the circumstance that the same subject is the object of several sciences certainly does not render any one of these sciences unnecessary. Plants are not exclusively dealt with by botanists; pharmacologists and chemists are concerned with them too. For ever so long geography has developed a method of objectively representing the surface of the earth, which is universally applied—the map. That the modern map contains much that is geodetic has been mentioned before. The need for good maps has been a mighty factor in advancing geodesy. A mere glance at a map will make us realize that it is something peculiarly geographical.

The map has often been designated as a picture of the surface of the earth. Not all the features of the latter, however, are reproduced by it, but only some of them, selected in each case. The selection is not the same everywhere. American topographical maps look different from European ones, even when we compare types using the same method of topographical representation by contour lines. On American maps the reproduction of the surface shapes is the prominent feature. They form the firm skeleton. The road system is frequently quite indifferent, and on some of the older sheets we may not only fail to find some railroads

but even whole towns which have come into existence since the map was surveyed. On the other hand, on the great European maps, roads, places, and areas under cultivation will be found to be reproduced in minute detail. Forests, pastures, fields, vineyards are most accurately distinguished from one another. The difference is not only due to the fact that in America, at least in the United States, topographical maps are made to serve as a basis for geological maps, while in Europe they were created for military purposes; but another reason is that in the civilized European countries the transformation of the primitive land into land under cultivation has been completed and accordingly the relative locations of fields, forests, pasture grounds have become almost permanently fixed, whereas in America that transformation is still in progress. Here the map often ignores what is still unsettled and in the making, and instead brings out prominently what is permanent in the surface features. These serve for orientation, whereas in Europe, roads, villages, and even isolated single buildings are used for that purpose. It is quite plain that as the change from the primitive landscape to cultivated land proceeds, American topographic maps will become more and more similar to European ones; and this does not escape the notice of attentive observers.

The making of geographical maps, however, is rarely left in the hands of the geographers themselves. In Europe it is mostly done by military men, in America by topographers. In the preparation of a map professional skill and routine are more important than scientific research, and the laying down of the observation is for the map of greater value than its interpretation and explanation. The making of maps and charts can be safely left to assistants, just as analyses are left to them by chemists, provided those so employed are conscientious and well trained; and if everything connected with the preparation and execution of the map is done in that truly geographical spirit which distinguishes the essential from the immaterial. The necessity of providing good and reliable maps of the civilized countries, which was by no

means merely due to geological and military, but quite as much to general public needs, has furnished the professional geographer with an exceedingly rich and valuable store of well recorded observations from which he is enabled to draw a great variety of conclusions. From the very nature of his work the topographer considers it as finished as soon as he has recorded the objects of his survey. This explains why it is that mapping so rarely leads up to more universal points of view. These are left to the geographer. But he must not restrict himself to obtain his knowledge of the surface of the earth from the maps. Even if he is no explorer and if the best maps of the territory to be investigated are available to him, he ought to go out and see himself, for his way of viewing things is thoroughly different from that of the topographer. The latter's business is to lay down facts on the map, and only too readily will he get into the habit of accepting them as definitely settled and immutable. The geographer, on the other hand, is aware that nothing he has to deal with on the surface of the globe is permanent, but that everything is constantly moving. The whole of what is to be seen on the surface of the earth is not a permanent condition but a state of momentary equilibrium. What he has to deal with are not mere phenomena but processes.

That even the solid rock structure of the earth's surface is not immovable was noticed early by good observers in antiquity, as mentioned before, and we have also explained the radical conclusions drawn therefrom with respect to evolutional history. Whether such morphological observations are made by the geologist or by the geographer, the fact must always be considered that parts of the earth's surface become covered by matter migrating along that surface, and are thus getting into the crust, whilst, on the other hand, the inside parts of the crust is constantly being laid bare, with the result that what pertained to the crust becomes the surface. It is quite natural to look upon these reciprocal changes as a normal process, especially as the ultimate effect of the wearing down as well as the sedimenta-

tion is the same, viz., the levelling of the earth's surface. But the forms produced by this levelling do not show a definite succession of types. Such is only the case where a quickly raised block is worn down in the course of long time. That, however, is not the rule. Crustal movements are acting everywhere contemporaneously with the levelling forces, and the surface forms are the resultants of the ratio of intensity existing between degradation and elevation in each particular instance. Two typical groups of forms occur: That of ascending development with convex slopes, where the raising exceeds the wearing down, and that of descending development, where the reverse is the case. Both types occur side by side, and the descending development may continue even in areas which have been elevated but are not yet reached by the ascending development which penetrates slowly in the raised block. Thus, thanks to the principles evolved by the morphological analysis of Walther Penck, we are enabled to draw conclusions from the forms of the surface of the earth as to crustal movements going on at the time in the region involved. Only few investigations are made in the new spirit, yet they reveal an unexpectedly great mobility of the crust even in regions which are regarded as relatively quiet. Numberless are the warpings, the low vault-like swells or shallow downbends that are going on, to say nothing of the more considerable elevations and subsidences revealed by mountain ranges and lowlands, and of the jerky lifts and sudden rifts caused by earthquakes. The earth is very mobile indeed. The actual crustal movements are not always reflected in the crust structure. The latter is the resultant of a whole series of movements which have not always operated in the same direction, but have often acted in opposition to each other, and the whole of them may present a different aspect from the last movement which is expressed in the surface forms. If the modern morphology of the earth's surface has taken over a good deal from geology, it now becomes an important aid in the investigation of geological processes by studying the earth's surface.

The evolutionary spirit of geology rules also in geomorphology. But the forms of the earth's surface cannot be arranged in such clear rows as those of the development in the organic world. There is not a growth from youth to maturity and old age, but a fluctuation as in the waves of the ocean, which swell up and work down between narrow levels.

Most intimately intertwined with the surface of the earth is its vegetable garment; in fact, it is an integral part of the surface. The topographical maps of the old world are paying due attention to it. No less a man than Alexander von Humboldt has pointed out that it determines the physiognomy of countries. The shape and the coating of the surface of the earth make up the essential contents of geography. But while the former is due to the action of endogeneous and exogeneous causes constantly operating against each other, the latter is the result of the soil and the climate, in the treatment of which geography and meteorology are sharing. With unfailing promptness the plants covering the earth respond to even the slightest climatic variations, being entirely dependent upon the solar energy which furnishes the warmth necessary for their growth, and provides the water they require. No less, however, are they influenced by the soil—not only the rocks coming to the surface of the earth, but more especially the products of their chemical transformation, due to weathering from the constant effects of the climate and vegetation. This soil, however, is even more mobile than the crust of the earth; it is moving everywhere, controlled by gravity; it creeps down the slopes of mountains, leaving their tops bare and accumulating at their foot. If we were to take a corporal conception of the earth's surface, we should have to regard it as the thin cuticle of soil, spreading over mountains and valleys, the bearer of the vegetation on the earth, and itself the product of a great number of organic forces. Eminent investigators, like Ferdinand Freiherr von Richthofen, have called the attention of geographers to this soil, but modern pedology has essentially been developed without the coöperation of geographers.

But no matter how plainly the vegetation covering the earth is subject to the influence of the climate, still it is not permissible, in studying it, to infer—as is done in physical geography—that like effects are produced by like causes and, reversely, like causes produce like effects. While a particular climate may be inferred from a particular kind and density of vegetation, still like climatic conditions do not necessarily produce like vegetation. The factors determining the maintenance and more or less luxuriant growth of plants are not sufficient to produce the latter. Where new land rises in the midst of the warm ocean, the climatic conditions exist for luxurious vegetation, but the latter fails to appear until plants can reach the new island. When they do, their growth may be quite extraordinary, but they cannot be produced there, in spite of most favorable conditions. The physical law of causality which otherwise is so eminently applicable in all sciences concerned with the earth, fails to work here.

The fact that organisms became integral parts of the surface of the earth, places geography in a class by itself among all sciences concerned with the globe. In a lesser degree, geology too, dealing so much with the remains of former life, is entitled to such classification. It is not a question of geography, as a science concerned with the earth, to solve the riddle of life. It has to accept the fact that life exists on the surface of the globe only where it can exist. To trace the roads followed by life in spreading over the earth is not necessarily one of the tasks of geography, even though it is able to contribute towards clearing up old land-connections. Animal and plant geography belong more to zoölogists and botanists than to the geographer. For him there remains a wide field of research on the physical conditions controlling life on the globe, and investigations on its distribution as a whole. The theater of life is part of the earth's surface and pertains to geography, and the manner in which especially the vegetation of the globe spreads over its surface will always be studied by geographers, because in

that way they obtain excellent characteristics of individual portions of the surface of the earth.

These characteristics refer to the shape, covering, and situation of individual spots of the earth; they call for a chorological manner of viewing things, which is not a peculiarly geographical one, but is applied also in regional geology, and in botanical ecology. But in no other science does it play so important a part as in geography. It consists in correlating an aggregation of various units, which is tied to a particular spot of the earth, or a definite region; in recognizing a balanced state of processes which are determined by the motion of the underlying piece of crust, the active solar energy and the accessibility of life. These processes are not only dependent on the locality involved, but processes originating in the neighborhood are playing a part. In this connection we should think of wind and water moving on the surface of the earth, of the precipitations falling in front of a mountain range. That is why the *geographical situation* is so very important in the method of regional investigation. In Germany the unit forming its basis has been termed "Landschaft." Investigations on the Landschaft are regarded by some as the real aim of a modern geography. The viewpoint however is not new, it has been used for scores of years, and not much has been gained by dividing a piece of surface into individual regions. Its peculiarity can only be fully understood and appreciated by studying the manner in which its individual regions are grouped, by considering what patterns they form, and by endeavoring to comprehend its geographical configuration.

The "Landschaft" (region) represents a state of balance, which is immediately disturbed if even a single one of the factors determining it is changed. No single factor is more effective in determining the character of a landscape than man. He roots up trees and clears forests, changing them into fields; he embanks rivers or distributes their waters through irrigation channels; he builds roads, settlements, towns. He changes the virgin land into cultivated land.

He transforms Naturlandschaft into Kulturlandschaft. In many parts of the United States this transformation is still in progress, in the civilized countries of Europe it is practically completed. Harmony has been restored, of a different kind, it is true, than that which prevailed in the virgin landscape and was disturbed there by the interference of man.

In view of the fact that now the cultivated area extends over millions of square miles, there can be no doubt that man, too, by his work is a proper subject for geographical investigation and that, aside from the physical processes, which are bound up by a rigid law of causality, and the biological phenomena not completely subjected to the same law, geography has to deal also with the actions of human will, the causes of which are often so difficult to understand. This wide span of different causalities is a fact which, in the view of many, deprives geography of unity. Wide-spread, indeed, is the belief that it is a dualistic science, part of which should be classified with the natural, and part with the intellectual sciences. This view, however, if founded not on the subject matter, but on the manner of treating it. Nearly every science starts from an anthropocentric manner of looking at things. Natural science begins with the distinction between useful and harmful plants and animals; practical surveying leads up to geodesy, mining to geology. Orientation on the surface of the globe is the root of geography, which was primitively the science of the world surrounding man. Herodotus looked at mankind from the viewpoints of space and time. In their beginnings geography and history keep close together; necessarily they diverge more and more, as geography objectively devotes itself to the surface of the earth. But its objective view has to deal with the fact that the terrestrial surface is the theater of life for the same human race which is the central object of history. Both sciences continue to keep in touch with each other, but this touch is no longer rooted—as it primitively was—in the anthropocentric point of view, but in the fact that human activity affects the surface of the globe by modifying it.

We can readily comprehend the geographical activity of man, because our sympathetic feeling enables us fully to appreciate the actions controlled by his intention. But we ought not to shift our viewpoint. If we lose the surface of the globe from our vision we fall back into a sort of anthropocentric view. That is what even eminent geographers have felt as dualism of geography. It has no material foundation, yet it persists in manifesting itself. Again and again it is attempted to remove geography from among the other sciences concerned with the earth and give it the old place by the side of history.

These attempts are by no means merely the result of a certain carelessness, but they are principally due to the fact that, by the side of the well and firmly organized system of the sciences investigating the earth, there is no corresponding system of the science of man. Only too long there has been only the study of his history, which gave the political states a prominent place. Much later only did his economy receive attention, and still later came ethnography and anthropology. But these sciences of man got little touch to geography. Here a gap has always been left. Ratzel tried to fill it from the side of geography by devoting to it the first part of his Anthropogeography. It deals with the application of geography to history. The second part Ratzel devoted to the geographical distribution of man. But the geographical activity of man on the surface of the globe and his geographical dependence in respect of his whole existence is still to be treated. A physical anthropogeography is as yet outstanding. Its viewpoint must be purely geographical; it could furnish most important principles to the science of man. For, no matter how much individual action may often appear as the expression of the human will, still mankind as a whole, like life as a whole, is subject to iron physical laws. Whatever the origin of life may be, its existence depends on a supply of food and, in the last analysis, on a supply of solar energy. And the spreading and increase of the human race is limited by the limited size of the earth. From this standpoint, geography obtains high importance not only for history,

but for all sciences of man. It shows the boundaries of his existence and the limits of his will.

The dualistic character of geography will disappear as soon as geography will give its attention to the great tasks of physical anthropogeography, and will leave much of what it now deals with as economic and political geography to the science of man, which will have to be further developed and expanded. In this connection it may again be pointed out that the investigation of the spreading and distribution of things over the surface of the earth is not yet geography. As it is not the task of geography to trace individual animal and plant species on the earth's surface, so it is not necessarily its province to investigate the mere spreading of man and his works on the earth. Undoubtedly, it is necessary to do this. But only as long as these important tasks are not taken up by other sciences can they be regarded as belonging to geography. Geographical instruction at school calls for digressions into other fields, similarly as biological instruction necessitates excursions into zoölogy and botany. Strictly speaking, geography as a science, has to deal exclusively with things connected with the surface of the globe. That is its clearly defined object. Now, as there are geographers who, in addition to one, devote themselves to another of the sciences concerned with the earth, so others could certainly cultivate a science of man in addition to one regarding the earth. While the two objects with which they are concerned are quite different, such scientists are not likely to look upon this as a dualism of geography, but they will simply feel that they are taking up two different fields which are lying near and are in touch with each other. They may do this with propriety, provided they are at home in both of them. A dualism is felt only by a man who sees the lines of demarcation between individual sciences rather than their points of touch, and who more clearly recognizes the distinction between the intellectual and the natural sciences than the intimate connection existing between all the sciences, and the fact that they are all closely related to one another as parts of a

grand, universal science. These parts do not lie side by side in a plane, like the different countries on a map, but they are closely connected and intertwined in many ways. Geography is not only in touch with other sciences concerned with the earth, but also with those concerning life and man. That does not deprive her of her character as a science of the globe. Through the uniformity of her field of activity she is enabled to concentrate her research; through the great number of her various points of touch she is able to coöperate in the development of the great, consolidated, universal science.

UNIVERSITY EDUCATION IN THE UNITED STATES

By JAMES R. ANGELL

(*Read April 30, 1927*)

The Two Hundredth Anniversary of this learned body, celebrated so auspiciously in this historic spot, inevitably occasions a retrospective and contemplative mood and, with that, brings a disposition to take stock of current assets in the world of intellect. It has therefore seemed to me fitting, in response to the courteous invitation of your officers, to dwell for a little upon certain aspects of American University education, which has undergone such extraordinary development since Colonial days and especially in the last half century. The purposes for which the American Philosophical Society was established are so closely related to the essential aims of our universities, that no achievements of the latter can be viewed with indifference by the former.

The American colleges of the seventeenth and eighteenth centuries, from which our universities have arisen, were naturally patterned directly upon their English originals, and only slowly broke away from this pattern as financial pressure and shifting social conditions compelled such change. The early Colonial colleges at least in New England, as is well understood, were founded predominantly to recruit and train a literate ministry, and throughout the first century of their existence a very large proportion of all their students entered this profession. The curriculum, much of it of an extremely elementary character, was frankly colored by this ministerial interest, and Greek and Hebrew, which constituted no small part of the educational fare offered, were originally designed solely to permit the reading of the Old and New

Testaments in the original tongues. The students were relatively much younger than those in our colleges of to-day, and the entire tone of the institution was rigid, austere and paternalistic.

Toward the end of the eighteenth century, the tendency to set up separate schools of medicine, law, and divinity, in parallel with the college, came into vogue and the university, as we know it, dates from this period. To many persons a university means simply a college of arts, literature, and science, surrounded by a group of professional schools, more or less inclusive in character, and this quite without regard to the educational level upon which the work of these various schools is conducted and with little or no regard to the ideals which inform them. Needless to say, in the face of so many shining examples as the United States presents, an institution is not made a university merely by the inexpensive device of calling it so.

The element which contributes the feature most characteristic of the University in the continental sense, puts in its definite appearance in the middle of the nineteenth century at Yale, in the special provision for what we now call graduate studies. To be sure, there had, from the beginning, been in many colleges an occasional Bachelor of Arts staying on at the college to pursue further his intellectual interests. But little or no attention was given to such persons, and the tendency itself had been sporadic. The trend toward the development of advanced post-graduate work, once it was begun, spread with reasonable rapidity. The founding of the Johns Hopkins University in 1876 greatly augmented the movement and the establishment of Clark University and the University of Chicago at the beginning of the last decade of the century also markedly stimulated it.

The graduate school in its most creditable form is committed to the maintenance of the highest standards of learning, to the stimulation and encouragement of productive scholarship and particularly to the advancement of the frontiers of knowledge. Where the college prides itself on

its predominant interest in the student, the graduate school is proud of its devotion to the subject matter of the studies which fall within its purview. It conceives itself as the peculiar guardian of exact and fundamental knowledge, of truth in its most significant sense. The term "graduate school" is not altogether happy, for it applies equally to professional schools open to holders of the bachelor's degree and it does not convey the significant implication of university, as distinguished from collegiate or technological, or professional, aims and methods.

Unfortunately—at least from some points of view—the graduate school has found itself exploited as a professional school for the training of teachers—a function which it has often served very badly. The difficulty would perhaps have been negligible if the training had been confined to persons who were anticipating a career as college or university teachers. But the obligation has been extended to persons expecting to serve in secondary education as well, and this fact has led to an inevitable lowering of the ideals of much of the work conducted under the auspices of the graduate school, and especially in connection with its conferring of the master's degree, which in most institutions is to be secured by a single year of post-graduate study, and is sought in overwhelming numbers by many persons quite devoid of the true university spirit. I do not share the frequent and often acidulous academic disdain of teacher training, which I regard as essential; but I regret that the graduate school has not as yet more perfectly adjusted itself to the somewhat divergent obligations under which it now labors. Nevertheless, with all its shortcomings, with the difficulty of sharply differentiating its standards and aims from those of the college, in the face of inability to command funds for the proper financing of its programme, and despite the attacks launched by reactionaries of every kind against its supposed pretentiousness, and the occasional pedantry of its doctors' theses, despite the pathos of much of its pretence of research, the American university derives its most vital intellectual

force from ideals which are most fully realized in the graduate school.

The college, in the nature of the case, is primarily a teaching institution, with its interest properly centered on the individual student and his development. To be sure, we have been passing through an interesting movement, largely borrowed from the procedure of the older English universities, whereby, under various titles, we are encouraging the ablest students to strike out for themselves in intellectual fields of their own choosing. The independence of outlook thus recognized and encouraged, leads naturally into the true attitude of inquiry and research and in so far is in reality the beginning of the genuine university attitude of mind. Indeed, the college has always to some extent reflected this attitude and at least one well considered program looking to this end was tested for over a decade in the University of Michigan before the end of the nineteenth century. The university and the college thus naturally shade into one another and are not to be thought of as standing in hostile isolation the one from the other, however necessary it may be that they stress somewhat different ideals.

The professional schools, however, until a relatively recent date, presented quite a different picture. In so far as the professional school conceives itself as set merely to teach a special technique, to produce a special kind of artisan, it is in no very worthy sense an intrinsic portion of a university. It may forsooth cherish ideals which are fundamentally at variance with those of the true university. It may be in essence a trade school—a very excellent thing, but alien to the university.

But if, in addition to conserving true learning, it regards its duty as in large measure that of advancing knowledge in the field of human interest to which it is especially directed, and if it trains men to think constructively for themselves and to strive to advance a deeper and more penetrating understanding of the phenomena with which it deals, whether physical or human, it is in so far a worthy member of the university organization.

Medicine has, in our time, made notable advances in this direction. Not only are the underlying physical and biological sciences conducted in a superb spirit of open-minded search for new truth, the clinical branches of medicine are also now widely infused with the same spirit. And in the measure in which this is true, the young physician instead of being trained by rule of thumb to use stereotyped remedies and procedures for standardized diseases, is rather being taught to treat each case as a subject for thorough and careful scientific examination and diagnosis, and he is constantly on the alert to reach some deeper level of understanding of the circumstances with which he has to deal. Even more important, there is growing up a generation of men—and institutions—dedicated to the relentless pursuit of disease, that its ultimate causes may be discovered and destroyed.

Law has been more lethargic in achieving a university point of view. But progress has been very rapid in the last few years and all the better schools are now conceiving their obligation to research and to a fresh and fundamental interpretation of the basic principles of the law as not less binding than their duty to send out competent craftsmen to serve the public and the courts. However it may be in other lands and under other civilizations, there can be no question that in our own country there is no more crying need than a radical reconsideration of the various procedures upon which legislation is based, under which the bench and bar are recruited, and through which they execute the mandates of the legislative powers. No step in any one of these directions can be safely taken without the most dispassionate and thoroughgoing study which the ablest of our minds can bring to bear upon this baffling complex of human experience and usage.

The whole conception of the relation of law to the structure of society demands fresh analysis and convincing exposition. Our Anglo-Saxon ancestry is supposed to afford us as a nation an almost religious veneration for the safeguards of

legal procedure. But if there be one social phenomenon more conspicuous than another just at present, it is the light hearted manner in which people from every walk of life flout the law and scoff at its mal-administration. It is cynically said that lawyers are valued almost as much for their supposed ability to advise how safely to circumvent the law as for their services in protecting person and property from illegal violation. The most fundamental postulates on which our jurisprudence rests require a fresh justification in the public mind, and this can only be brought to pass by men who have been schooled in the soundest methods of research into the social, psychological and historic foundations of the law.

Legislation is equally, or even more, in need of the control which can only spring from thoroughgoing investigation and analysis. Our courts are choked with the consequences of ill-considered statutes, often aggravating a thousand-fold the evils they were created to relieve. Our schools of law cannot be deaf to these calls for the broadest and most exhaustive scholarship, dealing in the spirit of impartial scientific objectivity with these multifarious perplexities which confront our courts and legislatures, our honest lawyers and the wayfaring man who still has a tenacious prejudice in favor of law and order and decency.

The practice in our divinity schools has varied very widely in the different seminaries. In the stronger ones, basic scholarship has long been recognized and honored, and research in every related field is fostered and encouraged. Indeed, there are not a few branches of learning in which the universities have looked to the divinity faculties for their best, and sometimes for their only, advanced work. This has been true, for example, in such fields as those of the Semitic languages and in the ancient civilizations of the East. But, on the other hand, not a little of the procedure in the seminaries has been highly dogmatic and conducted in a mood distinctly hostile to that of the open-minded spirit of inquiry which is of the essence of the university. While

loudly proclaiming their complete devotion to the truth, the seminaries have not unnaturally often insisted that what they taught WAS the truth and that research must not be allowed to lay upon it a profaning hand.

The better schools of law, medicine, and divinity are largely graduate in character—in the sense that they only admit to their courses, or confer their degrees upon, men who already have a bachelor's degree. It should therefore be easy for them to cultivate a real university attitude in that they have a student body intellectually prepared to understand and share in the promotion of these university ideals, and it should be a reproach to them when they fail to take advantage of this situation.

Technology occupies at the moment a somewhat ambiguous position—and I include in this term all those branches of study which are concerned primarily with the application of science to the practical problems of life in any of its phases. Generally technology is conducted as an undergraduate enterprise, in parallel with the arts college, and, as such, it conceives teaching and the individual student as its prime obligation. But some technological institutions are confined to more advanced students. In either case, like the college, one must hope that somewhere before the end of the course, the university spirit may be introduced, that the student may be led to an inquiring attitude of mind, challenging him to test accepted fact and then to proceed to try, as opportunity permits, to advance the knowledge of his time. It is only fair to say that in not a few institutions these ideals are already in some measure realized.

One may also venture, in this connection, to wish Godspeed to the tendency, in recent years increasingly discernible in our technological schools, to stress fundamental science and to reduce the attention earlier given to the technique of practice, whether in shop or field. In the degree to which emphasis swings to the fundamental sciences, does it become easier to foster the university spirit and outlook, and even in the field of so-called applied science, there is nothing to

prevent the development of this attitude, although it is certainly apt to be more difficult. But only as we approach the university ideal can we have any assurance that our technology will not slip back into the ideals and procedure of the trade school—as I have elsewhere said, an excellent institution, but not at home within the walls of the university.

Time does not permit that I should attempt to trace the changes which step by step have accompanied the emergence of the higher educational institutions of our day from the Colonial college of the seventeenth century, much less the manner in which American education has, from time to time, been affected by the direct importation of English, French and German ideals. But I venture to characterize briefly a few features of our national accomplishment in the struggle to develop a true university.

It would be ungracious and ungrateful in any way to minimize the service which the early American college performed. It was, for generations, the only intellectual light shining in dark places, and it sent out into church and civil state some of the ablest minds that our nation has produced. But it would be equally unwarranted to accord it a very high place for its achievements in the advancement of knowledge. It was, and still is, predominantly a conservative rather than a creative intellectual agency. For the first two centuries of its history, it was chiefly marked by the increased flexibility and the slow enlargement of its curriculum, by the introduction, for example, of the modern languages, of physical science, of history, and the social sciences, by modifications of its teaching methods, and finally at the end of the period by the outcropping of the university spirit of intellectual independence and the craving for new truth. This last spirit it is, most clearly exemplified in our graduate schools, which infiltrating into the professional and technical schools is giving our universities slowly but surely a true university atmosphere, an atmosphere reverential toward established truth and the methods of its determination, but hostile to unreasoning dogmatism and to intellectual

obscurantism of every kind, an atmosphere inquiring and skeptical of ill-founded assertion, but friendly and sympathetic to every sincere effort to advance knowledge, an atmosphere in which sound learning is venerated and pedantic sham and pretense despised.

Upon the uninterrupted development of this spirit rests in the last analysis the stability of all our education, from kindergarten to college. Knowledge is in reality fluid. Established fact requires constant re-interpretation and readjustment to changing conditions. Fresh knowledge must be knit into the fabric of the old, and the entire educational process must accommodate itself to this procedure. The maintenance therefore of the spirit of the university is no mere sentimental desideratum. It is of the very essence of intellectual self-preservation and there is no interest, social, ethical or political, which is not vitally affected by it. The true patriot may therefore properly hope that the American university will speedily come into its own as the most significant national center of light and learning, of art and science.

One does not like to close upon a minor key, but only harm can result from indulging an illusory and unwarranted self-complacency, and there are disturbing aspects of the whole situation which should be frankly recognized.

In the face of great apparent prosperity, higher education in this country, and especially the university, is gravely menaced by the difficulty—approaching impossibility—of recruiting an adequate amount of first rate intellectual ability to carry forward this great enterprise. To condemn it to the hands of men of second and third rate caliber would be nothing short of a tragedy. The circumstances are well understood by all thoughtful observers, but the general public has not been brought to a full realization of the situation and even our better informed classes are too largely oblivious to the facts.

The academic career cannot, and should not attempt to, compete in point of financial reward with business or industry, or even with the more highly paid professions. But unless

the conditions of academic work can be so adjusted that able and industrious men may at least look forward to reasonable comfort and dignity of life, to freedom from incessant financial anxiety and to conditions which afford adequate leisure for scholarly pursuits, instead of spending themselves, as is now too often the case, in an endless round of exhausting routine, we shall not find many of our best minds choosing the career of scholarship. They will prefer, often reluctantly, callings in which their talents are more justly rewarded. Even those of definitely scholarly tastes and capacities are dismayed by the financial sacrifices involved and especially as these may affect their wives and families.

We have the ideals, we have much of the necessary equipment, we have a nucleus of able men to guide development, but we must have a far more generous conception of what we owe to learning, and we must be ready as a nation to meet the costs. Any other attitude will inevitably spell defeat upon the very threshold of an opportunity such as no people ever before was offered. Let us hope that we may be found worthy of the priceless heritage which has been put into our hands; but let us not forget that it will be by works rather than by faith that we may expect to be justified.

DEMOCRACY AND THE CONTROL OF FOREIGN AFFAIRS

By WILLIAM E. LINGELBACH

(*Read April 30, 1927*)

The control and direction of foreign affairs constitutes one of the most serious questions of modern government, quite surpassing in importance most other problems of our time. Science and invention are eliminating distance and time, destroying physical barriers and making a little thing of this world. National isolation is a thing of the past. Peoples formerly remote from each other are to-day near neighbors, interdependent in their political, economic, financial, intellectual and even social life.

As a result international problems have tremendously increased in number and complexity. The records of our state department and of other foreign offices show an almost incredible increase in the number of despatches passing in and out year by year. Further they are vastly more important because mismanagement or failure in handling many of the problems of present-day foreign relations is apt to involve consequences much more dangerous than heretofore. The physical sciences are not only bringing the nations closer together, but they are putting into our hands engines of destruction so powerful that the results of their use stagger the imagination. It is therefore of the utmost importance that the democracies of the world understand how their foreign affairs are managed, and the extent to which control and revision may be exercised by the duly elected representatives of the people.

Until recent years most European foreign offices still labored under the antiquated idea that since foreign affairs had always been the exclusive prerogative of the crown,

they were so still, and therefore not subject to the rigorous control and supervision exercised by modern democracies over the business of other departments of the government. The gradual emancipation of the foreign office from monarchical domination, which took place in the second half of the nineteenth century, did not result in a corresponding control by parliament. True, the monarch was pushed aside and to all outward appearances parliament assumed control of the direction of foreign affairs. In reality, however, the practice in nearly every country put the actual control and management in the hands of the foreign minister without adequate provision for his accountability. As a result, the foreign policies of most European countries before the Great War were shaped by a small junta or camarilla, composed of the foreign minister, one or two fellow cabinet members, possibly the monarch and a representative or two of the military, commerce and high finance.

Not only did they keep the people and parliament in ignorance, but they frequently deceived them in order to win support for their policies. They made alliances in secret, spun webs of diplomatic intrigue and then, caught in the vicious systems they had themselves created, frightened colleagues and people into ever-increasing expenditures for armies and navies till the inevitable crash came in the World War. Selfish and braggadocio leaders and rulers blundered into the great disaster. But incredible as it may seem, the principal nations are still conducting their foreign relations much as before. To-day, after the experience of the most terrible war in history and the menace of scientific warfare in the future, secret diplomacy and an inexplicable fear of substituting coöperation and law for anarchy and war in international relations still prevails.

Speaking of the control of foreign affairs in England before the World War, Lord Curzon said, "Parliament knows little and the public knows less of what is going on." Treaties are negotiated and signed without the knowledge or consent

of the Commons and often remain secret for a long time. In France there are certain constitutional checks but they are not very effective and the Committee on Foreign Affairs of the Chamber of Deputies rarely asserts itself against the *Quai d'Orsay*. Just before the World War, the socialist leader, Jaurès, protested against the arbitrary conduct of the foreign minister. "It is a crime," he said "that treaties and obligations between the Republic and other states exist, which may involve it in a war, but of which the Deputies are in entire ignorance." In Germany, Austria-Hungary, and Russia, autocratic direction of foreign affairs continued into the years of the war. There was no democratic control. In the United States we are in the habit of pointing to our system of checks and balances and to the Senate Committee on Foreign Relations as evidence of congressional control and direction of our foreign relations. But when all is said and done, how much influence does it exercise in shaping and carrying out international policies? Our constitution provided for the democratic control of foreign relations a century before Great Britain and other powers did so, but in recent years, critics of our system claim there is a dangerous tendency toward autocracy in this department of our government.

The anomalous character of this situation had attracted the attention of scholars and statesmen for some years before the World War. During, and following, the great disaster, the subject assumed a new and unique importance. This was due, first, to the appalling consequences resulting from the mismanagement of foreign affairs, second to the establishment of the League of Nations, and third, to the unexpected and altogether unprecedented revelations of the secret diplomatic archives of the pre-war period.

Heretofore the records of the diplomatic history of all important countries were kept under lock and key for more than half a century after the events. It was impossible for the people to know what had been done. There was no opportunity to profit by past experience because the knowl-

edge of the past could not be obtained. Most of the histories of international relations written during the century following the events had to be based on such evidence as the different foreign offices were willing to publish and upon private memoirs, correspondence and diaries. Parliaments and people were kept in ignorance not only of what was being done but of what had been done. A veil of secrecy was spread over the past, as well as the present.

Evidence of this can be found in many of the important crises in the international relations of England in the last fifty years. During the Turkish difficulties, 1874–78, Disraeli and Derby decided that Parliament should not be troubled with the problems and, of course, not be given an opportunity to interfere.[1] Not only did the minister not consult the Commons, but he deliberately deceived them. Later in the negotiations, he sent the fleet to Constantinople, and concluded the alliance with Austria and the convention with Turkey without reference to Parliament.[2] The significance of this appears when it is recalled, that by the terms of the agreement, England pledged herself to guarantee Asiatic Turkey against Russian advance.

Salisbury was a trifle more disposed to take his colleagues into his confidence and to respect the claims of Parliament, though one cannot help having the suspicion that his action was, at times at least, dictated by a desire to protect himself. On the occasion of Bismarck's proposal for an Anglo-German alliance, in 1889, Salisbury replied that while he personally thought an alliance would be a blessing for both countries and for the peace of Europe, he and his colleagues found that it would break up the parliamentary majority and overthrow the ministry. "Unfortunately," he said, "we are no longer living in the time when the aristocracy ruled and we could pursue an active policy. Democracy is now the ruler and with it party government, which has made every ministry depend absolutely on the *aura popularis*."

[1] Moneypenny and Buckle, "Life of Disraeli," Vol. VI., p. 15 *et passim*.
[2] *Ibid.*, p. 248 ff.

In some respects this was true. But in general even Salisbury was in the habit of keeping Parliament in the dark when foreign affairs were concerned, and by no stretch of the imagination can it be said, that Parliament either directed or controlled English diplomacy during his administrations.

His successors, Lansdowne and Grey, adhered strictly to the old system. Indeed, the conspiracy of silence, if anything, became stronger. Attached to the Convention of 1904 between England and France in regard to Egypt and Morocco were five secret articles. When the main body of the treaty was made public, a private member, Mr. Weir, asked the Under-secretary for Foreign Affairs if there were any secret clauses attached to the Anglo-French agreement. The facetious reply made for the government by Lord Percy is typical: "Speculation and conjecture, as to the existence or non-existence of secret clauses in international treaties," he said, "is a public privilege, the maintenance of which depends upon official reticence."

The secret articles were not made public till 1911, when they were published by the *Temps*.

The same official reticence was maintained in regard to the important conversations and arrangements between the military and naval authorities of the two countries. These Grey inherited from Lansdowne but they were greatly extended during his direction of the Foreign Office. The nature and the extent of the conversations were kept strictly secret. Nor was Parliament or the cabinet informed when, after the second Morocco crisis, the verbal understanding was finally put into writing in the now famous Grey-Cambon letters. Someone in the inner group urged it for the sake of definiteness. Even then the secrets were carefully guarded by the select few against repeated efforts on the part of members of Parliament to get at the facts.

Lord Hugh Cecil addressing the Prime Minister, on March 10, 1913, said:

> There is a very general belief that this country is under an obligation, not a treaty obligation, but an obligation arising out of an assurance

given by the Ministry, in the course of diplomatic negotiations, to send a very large armed force out of this country to operate in Europe. This is the general belief.

Asquith's reply left plenty of room for suspicion. "I ought," he said, "to say that that is not true."

Several days later, on March 24, he was more emphatic, saying:

> As has been repeatedly stated, this country is not under any obligation, not public and known to Parliament, which compels it to take part in a war. In other words, if a war arises between European Powers, there are no unpublished agreements which will restrict or hamper the freedom of the Government or Parliament to decide whether or not Great Britain should participate in a war.[1]

Finally, however, when Grey under pressure of the events in August, 1914, read the letters to the Commons, the situation appeared in a somewhat different light. Referring to the question of British commitments, he said that he and the Prime Minister had assured the House more than once that in the case of a crisis, "We should come before the House of Commons and be able to say to the House that it was free to decide what the British attitude would be, that we would have no secret engagements which we should spring upon the House and tell the House that because we entered into that engagement, there was an obligation of honor upon the country. . . ."[2] After reviewing the development of the Entente he came in conclusion to the Grey-Cambon Letters, but in reading he omitted the last and vital sentence which reads:

> If those measures involve action, the plans of the General Staffs would at once be taken into consideration, and the government would then decide what effect should be given them.[3]

The significance of this either by itself or when taken in connection with the rest of Grey's policy does not concern us here any more than does the question of the justice or the

[1] "Parliamentary Debates," Series 4, V. 183, C. 994–1000.
[2] "Parliamentary Debates," Series V., Vol. 65, C. 1812.
[3] "British White Paper," Enclosure No. 1, in 105.

expediency of his policy as a whole. The question at issue from the point of view of this investigation is that at no time, not even at the last moment, was Parliament or the nation permitted to learn precisely what the relations of England to the continental alliances were.

In his *Memoirs*, Grey says the omission was unintentional, that it doubtless occurred because of an interruption at that particular point in the reading. It did not occur to him, he says, that any importance attached to the sentence. Be that as it may Parliament was kept in ignorance to the very last, just as it was on question of the Anglo-Russian negotiations for a naval accord in 1914.

Following the visit of the King and Queen, accompanied by Grey in April to Paris, negotiations for a naval convention with Russia along the lines of the Anglo-French agreements were begun. Through a leakage at the Russian Embassy, however, the facts got to Berlin and were published in the *Berliner Tageblatt*. Suspicions were aroused and Grey felt obliged to deny the rumors. Referring to the prime minister's statement quoted above he said: "No negotiations with any power had been or were being or were likely to be undertaken which would make this less true. If any such convention were contemplated which would modify the prime minister's declaration it would have to be laid before Parliament." "This ambiguous phraseology," says Dr. Gooch, "was interpreted in different ways; but the inability to meet a plain question with a direct negative confirmed the suspicious ones in their fears."[1] Reporting to Sasonov, who had expressed his annoyance at the slowness of the negotiations, Benckendorff said, on July 2, "I see no reason to believe that the government has the least objection to carry out the Paris project. If it is not yet finished, it is because Prince Louis is to complete the negotiations in Petrograd. Another cause of the delay is the indiscretions. Perhaps Sir Edward wishes that the disquietude in Berlin should diminish before he goes further. As a matter of fact, he would find it difficult at the same moment to issue a *dimentis* and to negotiate."

[1] Gooch, G. P., "Modern European History," 1878–1919, p. 529–31.

This relationship between Parliament and the Foreign Office during the decade before the World War in the matter of the control of foreign affairs was admirably summed up by Lord Curzon in 1908:

"We concede, and I think rightly concede here in this country a wide and almost unlimited discretion in treaty-making to our governments. They conduct their negotiations in absolute secrecy. Parliament knows little of what is going on, and the public knows less. From time to time a minister with artless candour may lift a corner of the veil and disclose the ravishing beauty of the object behind. In the present case we were denied even that privilege. The government has, it is true, power to consult, or not to consult, experts as they please, and I have noticed as a rather remarkable coincidence that treaties have a habit of being signed a few days after the rising of the House of Commons. In that way immunity from parliamentary criticism for at least six months is secured." [1]

Frequent efforts to reform conditions have occurred, from the long debate in 1886 to the defeat of Mr. Trevelyan's motion on the subject in 1925. The principal arguments can be readily found in the parliamentary debates on the different occasions.

In November, 1911, the House of Commons, after an earnest debate on the subject, passed a resolution directing its ambassadors abroad to report on the conduct and control of foreign affairs in the country to which they were accredited. The reports were published in a Blue Book entitled "An Investigation into the Control of Foreign Affairs." Some are excellent, and the series as a whole constitutes a very valuable review of the then current theory and practice.[2]

After the war the demands became more frequent, reaching a climax in the opposition measure introduced by Mr. Trevelyan in 1925, just referred to. The motion read:

"That no treaty shall be ratified and no diplomatic arrangement or understanding with a foreign state involving, directly or indirectly, national obligations, shall be concluded without the consent of Parliament, and no preparation for coöperation in war between the

[1] "Parliamentary Debates," Series IV–V., 183, C. 994–1000.

[2] "Parliamentary Papers," 1912–13, "Accounts and Papers." Misc. LXVIII. Cd. 6102.

naval, military or air staffs, and the naval, military or air staffs of a foreign state shall be lawful unless consequent upon such arrangement or understanding; and this Resolution shall be communicated to all states with which this country is in diplomatic relations and to the League of Nations." [1]

Of course the motion was lost, no one expected it to pass. If the Labor government while in power did not inaugurate reform, it could hardly be expected from the Conservatives.

The organic laws of France provide several checks which in theory reserve considerable control to the legislature.[2] In practice, however, it has proven very slight. This is true also of the rôle of the Committee on Foreign Affairs of the Chamber of Deputies, for only in very rare instances does it appear to have had any determining influence upon the policies and actions of the *Quai d'Orsay*. The circumstances under which the constitutional laws were adopted in 1875 were favorable to the creation of a strong executive department.[3]

By Article VIII. of the Organic Law "The President of the Republic negotiates and ratifies treaties. He communicates them to the Chambers as soon as the interests and safety of the state permit." This manifestly places in his hands the right to determine when the "interests of the state" make it wise for him to inform the legislature of conventions and treaties he may have negotiated. This at least was the thought of the commission as revealed by the following remark of the member who submitted the plan: "We leave it to him (the president) to judge the cases which demand secrecy." [4] True, treaties of peace and commerce do not become definitive until they have been voted by the two

[1] Cp. "Parliamentary Debates," Series 5, Vol. 181, C. 1430.

[2] Cp. Article 3 of the Law on the Organization of the Public Powers adopted on February 25, and Articles 8 and 9 of the Law upon the Relations of the Public Powers of July 16, 1875; Duvergier, Lois, LXXV., 42–53 and 250–255.

[3] The determination of the two Royalist groups to reëstablish the monarchy had been frustrated by the White Flag letter, but they did not for that reason accept the idea of a permanent republic. Even in the drafting of the constitution the idea of its availability in case of a monarchical restoration were kept in mind. Laboulaye in reporting the law on the powers of the Executive for the Commission on July 16, 1875, made a special plea for a strong executive.

[4] "Annales de l'Assemblée Nationale," 38, 223.

chambers. These are not, however, the treaties that really matter.

Treaties of alliance and military conventions which are from their very nature likely to be more momentous in their consequences are not subject to legislative veto. Of the two conventions with Germany in regard to Morocco, for example, only the agreement for the cession of Congo territory was submitted and not the Morocco Convention proper. The latter, to which it was only an annex, as it were, was kept secret. Treaties of alliance, says Esmein, are not included in the class that have to be submitted under Article 8. This statement cannot be accepted in its entirety, however, because there is the exception that "treaties involving the finances of the state" call for legislative action. Obviously, however, there is room for wide difference of opinion. Does it refer only to financial aid given to the other contracting party or also to liabilities of an implied character? Further the chambers cannot modify or amend treaties submitted to them, they can only approve or reject.

In the matter of declaring war and making peace the law is explicit. "The President of the Republic," says Article 9, "shall not declare war without the previous consent of the two houses." This would seem ample guarantee against executive action on this score. Yet in 1914 the question of war was not submitted to the Chambers. That Poincaré was anxious to avoid the necessity is well known. Germany's declaration saved him from doing so. France did not declare war "without the consent of the two chambers"; she did not declare war at all on either Germany or Austria. On the other hand she was bound by her engagements with Russia to do so and it is an interesting matter for speculation whether the government could have taken the offensive if Germany had held off. At any rate the Chambers would have been confronted with commitments made during a period of more than twenty years, and a *fait accompli* over which they had had no control.

There is a commission on foreign affairs of which the

British minister in Paris, in response to the request of his government, referred to above, reported as follows: "The Chamber of Deputies appoints sixteen grand committees at the beginning of each fresh legislature to examine and report on questions concerning the various departments of state with which the House is called upon to deal. One of these committees is the commission on foreign and colonial affairs. It is elected for the whole legislative period (four years) and composed of forty-four members, designated by the various political groups in proportion to their numerical strength. The choice has to be confirmed by the Chamber at a public sitting. The committee does not examine the Budget for foreign affairs, as the yearly credits for that department are discussed by the general Budget Committee (Commission du Budget). The foreign affairs committee reports to the chamber on all questions relating to foreign policies which are submitted to it. It may summon before it any persons whose evidence may be of a nature to guide it in its deliberations. When these persons hold an official post the minister concerned must first give his consent, with or without the condition that professional secrecy is to be observed. A minister himself may be requested to appear and give explanations to the committee on points of policy. Although, in theory a minister is not obliged to appear before the committee, his refusal to do so would not be in harmony with French parliamentary traditions. All papers which the committee desires to consult are communicated to it through its chairman by the competent minister. Should the latter consider that reasons of state forbid him to communicate any document thus asked for, the minister informs the chairman of the committee, who usually acquiesces in the minister's view. If the committee persists in its demand, the matter is brought before the Chamber. Should the latter support the committee such action is likely to bring about a ministerial crisis." [1]

[1] "British Parliamentary Papers," 1912–13, "Account and Papers," Misc. LXVIII., Cd. 6102. Pp. 7 ff.

But even the commission is rarely told of the more important commitments at the time they are made. The debates contain many complaints of the unwillingness of ministers to supply the desired facts. Speaking on the Franco-German treaty on December 14, 1911, Count de Mun said: "Doubts on our commitments to Spain would still exist, if, in the commission on foreign affairs . . . we had not undertaken to tear away, bit by bit, the veil of diplomacy and to cause to appear at last all these secret treaties of which we were ignorant when we came together." [1] While the commission may under certain circumstances examine witnesses under oath and pursue investigations even abroad, its competency to command the documents of the foreign office or compel attendance of witnesses is not generally accepted. Even under the law of 1914, witnesses can only be obliged to do so in inquiries specifically ordered by the chamber.[2]

The reports of the commission to the Chamber are far from being as complete as they might be. Very often they omit a good deal of material that came before it. Thus the project for the bill on the Congo Agreement of 1911, for example, has no mention of the negotiations with Germany which led up to the treaty, though it does give the secret articles of the Anglo-French and Franco-Spanish treaties of 1904.[3] The fact that it was not until 1911 that these treaties were finally made known to the deputies, and, as we know from Count de Mun's statement, not even to the commission until six years had elapsed, is another illustration of the lack of control by the deputies over the policies of the foreign office.

The Deputies like the members of other lower houses have, of course, the right to ask questions. Indeed, there is a special provision for this in the rules of both Houses, a clear distinction being made between oral questions and interpellations. The former cannot be asked without the consent of the minister concerned having been first ob-

[1] Cp. "Débats Chambre," 1911, p. 39172.

[2] *Journal officiel*, 1914, p. 2650. Cp. also on this general question Esmein and Barthélmy.

[3] Documents Parlementaires, Chambres, 1911, Sess. Extr. No. 1413, pp. 304–337.

tained, and then only at the beginning or the end of a session. They cannot lead to a general debate. The answers for the most part are unsatisfactory and beyond the control of the Chambers because the article specifically protects the minister if he declines to answer. It reads:

Les ministres ont la faculté de déclarer, par écrit que l' interêt public leur interdit de répondre ou, à titre exceptional, qu'il reclamant un délai pour rassembler les éléments de leur réponse.

Interpellations on the other hand can be made at any time, but have to be introduced by a "demand" in writing and signed by one or more members. It is read by the president of the chamber and a date for the discussion is fixed, usually within a month, save in the case of interpellation on foreign affairs which may be deferred for a longer period. Once up for debate, however, an interpellation may result in what is tantamount to a vote of lack of confidence and the overthrow of the ministry.

Plainly the rules give little encouragement to members to occupy themselves with foreign affairs. From a careful count made recently in connection with this study it was found that 5424 questions *écrites* were asked from April, 1910, to April, 1914. In June and July, 1914, during the brief session following the general election and just before the war, 338 more came in.

Strange as it may seem, however, only a small number were addressed to the Foreign Minister, and of these, all but a few had to do with minor questions. That is to say, during the years when the Republic was conducting the most important foreign negotiations of its existence, and making commitments of vital and far-reaching importance, the deputies were not only not aware of what was going on, but, being ignorant, were not even curious.

On November 26, 1912, M. Brisson asked the Foreign Minister to tell the Chambers "to what France is engaged by the terms of the secret treaty of alliance between France and Russia, and by those of the Franco-English Entente." The minister's reply was, that the "government cannot reply

to this question."[1] A week later Camille Reboul asked the Foreign Minister "whether he could make known the claims and the consequences of the treaty of alliance with Russia." He received the same reply.[2] In connection with the interpellations in March, 1911, Deputy Jaurès declared:

> It is a scandalous and monstrous thing that today in the European and world wide civilization, there are secret treaties between two great peoples which may be in contradiction to the will of the people themselves and to the stipulation of public law.[3]

In view of the inability of the Deputies and the Senate to obtain satisfactory information through questions and interpellations, it need hardly be pointed out that the Yellow Books (*Livres Jaunes*), which, like the English Blue Books and the German White Books, are supposed to furnish important information on diplomatic matters to parliament, are rarely published until long after the events. Even then they are surprisingly incomplete, containing only such information as the government considers it expedient at the time to publish. The Yellow Book on the Franco-Russian alliance, for example, was not published till 1918, twenty-seven years after the alliance was made. Even then none of the documents on the important military conventions after 1899 are included, despite the fact that these were specifically incorporated into the political agreement. The papers on the Franco-Italian accord of 1902 on the Morocco question were not given to the chambers until 1920. In connection with the debates on the Franco-German convention, signed on November 4, 1911, the promised Yellow Book was delayed despite a resolution in the Chamber calling for its distribution. The foreign minister, supported by the prime minister, objected because "the consent of foreign governments concerned must first be secured." These illustrations show clearly that the Yellow Books are apt to be of greater use to the historian than to the legislators in their consideration of current problems.

[1] Journal Officiel, Débats, Chambre, Dec. 2, 1912, p. 2915.
[2] *Ibid.*, Dec. 12, p. 10402.
[3] *Ibid.*, March 24, 1911, p. 1436.

The constitution of imperial Germany provided, among other insignificant checks, a consultative committee on foreign affairs of the Federal Council presided over by Bavaria. Its control over the foreign policy, however, was quite unimportant. The Emperor exercised autocratic power in the conduct of foreign affairs, although in practice a great deal of latitude seems to have been left for considerable periods of time at least, to the Chancellor and influential persons like "His grey Eminence," Holstein of the Foreign Office.[1] Every now and then the erratic eruption of the Kaiser asserting his imperial prerogative makes its appearance. The outstanding examples are, of course, the *Daily Telegraph* interview, and the Kruger Telegram. Another of a more intimate nature appears in the extraordinary marginal comments on certain documents relating to the Serbian crisis during the fateful days of July, 1914. Against the statement that his Ambassador at Vienna had urged moderation on Austria, he wrote "Who authorized him to do this?" On Lichnowsky's dispatch from London the Kaiser's irritation finds expression in such drastic phrases as "The Serbs must be finished as soon as possible." "Serbia's national dignity does not exist . . . the question has nothing to do with Grey; it is his Majesty Francis Joseph's affair. What colossal British impudence." [2]

Equally instructive in this connection is the "Willy-Nicky Correspondence," and the Björkoe agreement in 1905. By the latter the two Emperors bound themselves and their respective governments to such an extent that when the Russian cabinet was informed the ministers refused to give their consent. Count Witte bluntly asked the Tsar if he

[1] Von Eckhardstein remarks in his "Ten Years at the Court of St. James" that the true story of Germany's foreign policy will never be known because historians will fail to take account of Baron Holstein's activities and influence. Perhaps because of this warning, Holstein's rôle has received very searching attention and we are fairly clear on his share in the conduct of the German Foreign Office. With Bülow he sought the rapprochment with England while the Kaiser favored an understanding with Russia till the rejection of the Björkoe accord showed the futility of efforts in that direction.

[2] Karl Kautsky, *Comment c'est délanchée la guerre mondiale*, pp. 53, 131. Scores of similar examples are found in the volumes of "Die Grosse Politik der Europaischen Kabinette," 1871–1914.

had forgotten that he had an alliance with France. After much hesitation Lamsdorff was authorized to telegraph Berlin that the Björkoe agreement could not stand. To the German reply that it had the signature of the Tsar, the only answer was a repetition of the telegram that the agreement could not stand.

Here we have an interesting example of the strength of the Foreign Office against the autocratic power of the Tsar. Nominally Nicholas II. directed Russia's foreign policy. In reality, however, the ministers were stronger. Yet it was only the chance of the ministers standing firm that kept a personal exchange of courtesies between the two autocrats of Germany and Russia from changing entirely the direction of the foreign policies of the two empires. For once the will of the Tsar was unequal to the task of coercing the Cabinet. The ministers had their way. The Russian foreign office had come into its own.

The despotic, almost capricious management of Austria-Hungary's foreign affairs before the World War is well known. Berchthold's delays in order to prevent Germany's suggestions that Austria moderate her policy at Belgrade from reaching Francis Joseph while there was still time to act, was exposed soon after the war. Neither the Austrian nor the Hungarian parliaments had anything to do with the minister's policy, and the Delegations left matters largely to the ministers and the ruler. Berchthold's action is interesting, however, as an outstanding example of the power possessed by foreign ministers and executives to create a situation which may make war inevitable.[1]

In this country we flatter ourselves that we have a constitution more or less fool proof on this score. Considering the time and the circumstances of their adoption, the provisions for the control of foreign affairs are remarkable. Even to-day, despite the demands of new conditions and the growing complexity of international problems, the system

[1] The story of the Austro-Hungarian negotiations following the assassination of the Archduke Franz Ferdinand are discussed by Gooch, Fay and others.

set up by the founders seems reasonably adequate. Certainly it continued for a long time the most enlightened and effective effort at democratic control of foreign affairs in existence.

In view, however, of the attention given to the subject since the war, and the interesting plans incorporated in most of the new constitutions of Europe, there are those who claim that our own system is in need of radical revision. Certainly we no longer stand out conspicuously in the forefront of the movement for democratic control. We have a Senate Committee on Foreign Relations, senatorial veto on diplomatic appointments, and ratification of treaties by a two-thirds vote of the Senate. Congress alone can declare war. It controls the purse and, as a last resort, it may impeach the President if he oversteps the bounds of his constitutional powers. But as every student of American diplomatic history knows, the sphere of Executive action is not only extremely wide, but despite these checks, it often oversteps the prerogatives conferred upon it by the constitution.

The problem of foreign affairs thrust itself early upon the leaders in the Revolution. In 1775 a secret committee of correspondence was appointed with Benjamin Franklin as its chairman.[1] Later Congress created a Committee on Foreign Affairs. Under the Articles of Confederation in August, 1779, a Department of Foreign Affairs was created with Robert Livingston at its head. But he was a secretary of Congress rather than an executive officer.[2] Congress really acted as a committee of the whole. It consulted the French ministers and took an active interest in foreign relations, especially in the instruction to the commissioners in Paris—Benjamin Franklin and his colleagues, Henry Laurens, John Jay and John Adams.

In the Constitutional Convention several plans were suggested for the permanent management of foreign affairs. An early draft gave the treaty making power to the President

[1] Wharton, F., "Revolutionary Diplomatic Correspondence," II., 61.
[2] Wharton, F., *opus cit.*, I., 251.

subject to the advice and approbation of the Senate.[1] Pinckney's plan, on the other hand, gave much larger powers in the arrangement of foreign affairs to Congress. It stipulated that, "The Senate shall have the sole and exclusive power to declare war and make treaties and to appoint ambassadors and other ministers to foreign lands and judges of the supreme court."[2] A modification advocated by Hamilton, provided that, "The Senate shall exclusively possess the power of declaring war. No treaty shall be made without their advice and consent."[3]

The final result of the debates and discussions of the different projects are embodied in the following paragraph of the Constitution.

"He, the President, shall have power, by and with the advice and consent of the Senate, to make treaties, provided two-thirds of the Senators present concur; and he shall nominate and, by and with the consent of the Senate, shall appoint Ambassadors, other public Ministers and Consuls. . . . He shall receive Ambassadors and other public Ministers; he shall take care that the laws be faithfully executed and shall commission all the officers of the United States."[4]

Two years later by an act of July 27th Congress created the Department of Foreign Affairs, later in the same year renamed the Department of State. To it were assigned "correspondence with and instructions to diplomatic and consular agents abroad, and negotiations with the agents of foreign nations in the United States, or such other matters regarding foreign affairs as the President of the United States shall assign to the said department."

This together with the significant provisions in Art. I., Sec. 8, completes the legislative coöperation with and the checks upon the Executive power over foreign affairs.[5] It gives to Congress the power to lay and collect taxes, to regulate foreign commerce, establish a uniform rule of naturalization . . . to declare war . . . to raise and support armies . . . to pro-

[1] Farrand, Max, "Records of the Federal Convention of 1787," I., 292.
[2] *Ibid.*, III., 599.
[3] *Ibid.*, III., 622. Also Jameson, J. F., Studies in the History of the Federal Convention of 1787, pp. 143–150.
[4] Article II., Section 2, Par. 2, Constitution of the United States.
[5] Cp. The excellent treatise by Z. Wright: The Control of American Foreign Relations.

vide and maintain a navy . . . to make laws . . . for the carrying into execution of the foregoing powers and all other powers vested by this Constitution in the government of the United States or in any department or office thereof.

In these regulations we have not only the earliest, but the most practical provisions for the democratic control and direction of foreign affairs. Curiously enough, however, in actual practise the tendency to center more and more power in the hands of the Executive is growing, and with it a disposition to hedge about the State Department with the customary diplomatic secrecy. Indeed, there are significant cases in our recent history where the Executive, following close upon the European custom of making commitments in secret, has created precedents, which, if persisted in, bid fair not only to brush aside the legislative control established by the Constitution, but to involve us in complications quite foreign to the wishes of the people. Again and again in the history of our international relations dangerous situations have been created by the independent action of the Executive; gentlemen's agreements have been made, conventions like that with San Domingo, and actual usurpations as in the case of Panama, have been executed without consulting even the Senate Committee on Foreign Relations. Commenting on the San Domingo affair, Mr. Roosevelt said, "The Constitution did not explicitly give me power to bring about the necessary agreement with Santa Domingo. But the Constitution did not forbid my doing what I did. I put the agreement into effect, and I continued its execution for two years before the Senate acted; and I would have continued it to the end of my term, if necessary, without any action by Congress. All that they (Senators) had really done was to shirk their duty. Somebody had to do that duty, and accordingly I did it."[1] If there is any doubt as to the seriousness of such action, it will surely be quickly dispelled by a perusal of the terms of the understanding with Japan in 1905 as revealed in the Roosevelt-Lodge papers.

[1] *Theodore Roosevelt, An Autobiography*, pp. 510–11.

According to these, President Roosevelt in 1905, through his personal representative, definitely assured the prime minister of Japan, Count Katsura of our coöperation with Great Britain and Japan in the Far East. An "agreed memorandum" of the understanding was formally endorsed on July 31 by the President as follows: "Your conversation with Count Katsura absolutely correct in every respect. Wish you would state to Katsura that I confirm every word you said." [1] The agreement involved among other things, an assurance by the Japanese of peaceful intentions in regard to the Philippines, we in turn agreeing to acquiesce in the Japanese policy toward Korea. Yet our ambassador to Japan, Mr. Lloyd C. Griscom, was not informed, nor did the Senate Committee on Foreign Relations know anything about it. On the latter the President expressed himself forcibly on May 6: "As to what you say about the alliance, the trouble is, my dear Kennan, that you are speaking academically. Have you followed some of my experience in endeavoring to get treaties through the Senate? I might just as well strive for the moon as for such a policy as you indicate. . . ."

Nevertheless he committed the nation to a definite policy to which he would have been morally bound to adhere as long as he was in office, whether Congress or the nation approved of it or not. Altogether the memorandum constitutes one of the most remarkable executive commitments in American history.

Equally instructive from the point of view of this discussion is the now famous voyage of the fleet around the world. In a way, it put to the test the indirect control of our foreign affairs assigned to Congress through appropriations. There was, it will be recalled, money to send the fleet to the Pacific, and the President, with a humorous sense of the realities in the situation, significantly said, if Congress didn't appropriate funds to bring it back it would stay there. While this is

[1] Cp. Tyler Dennett, "President Roosevelt's Secret Pact with Japan." *Current History Magazine*, October, 1924.

not exactly controlling our foreign relations, it comes very nearly doing so. Even the Cabinet was not consulted in the matter. Mr. Roosevelt said later: "I determined on the move without consulting the Cabinet, precisely as I took Panama without consulting the Cabinet."[1]

There is, of course, always the power of impeachment in cases where the Executive is too arbitrary in his defiance of Congress. In general, however, the President as the elected Executive may be counted on to reflect fairly closely the wishes of the people. Moreover, he is better able to know the pulse of the nation than is Congress, and if he is judicious in building up public opinion through the press and other powerful agencies at his command in these days, he can nullify most of the constitutional limitations upon his power in the control and direction of our foreign affairs.

Even so careful an Executive as President McKinley in 1900 acted in China without Congressional authority, ordering our troops to coöperate with those of other powers in the storming of Tien Tsin and the march on Peking. Congress and the people had no voice in determining McKinley's policy any more than they had in controlling what really was an exercise of the war powers of the president in time of peace. Further, the treaty subsequently made with China seems never to have been ratified by the Senate.

Addressing a small group of speakers recently, Senator Borah frankly acknowledged that it was quite within the competence of our State Department to create a condition in our international relations that would make war inevitable.

Why usurpations of this kind by the Executive are not more often repudiated is a matter of considerable interest. The answer, obviously, is that Congress and people are usually not informed in time. When they do learn of the commitments it is often too late to effect a change of policy; it becomes a matter of patriotism to fall in line. The consequences of reversing or repudiating the action of the Executive would be too serious.

[1] *Opus cit.*, p. 548.

Occasionally, however, when Executive acts go too far in committing the country, or are too violently out of accord with the traditions of our foreign policy, public opinion may force their revision or even their complete renunciation. The most outstanding example of this occurred in connection with President Wilson's policies at Paris during and immediately after his first visit in 1918. For reasons now well known, strong opposition to his plans, notably on the League of Nations, developed, and a powerful propaganda against the President and his policies was inaugurated. Whether for good or for ill, the results were of tremendous significance both at home and abroad. We did not agree to the treaty of Versailles, the Senate did not ratify the security pact with France, and we did not join the League of Nations, despite the fact that our war President had openly and in person committed the nation to all three. Here at least is an instance where the constitutional inhibitions upon Executive power were brought into play. What the state of our foreign relations and those of Europe and the world would be to-day had the usual course been followed in the conduct of our foreign affairs in 1918, makes interesting food for speculation.

The new constitutions of Europe all make provisions for a measure of parliamentary control and supervision. In the main they provide that treaties in order to be binding be ratified either by the legislature itself or be passed upon by a standing committee on foreign affairs. In Norway the procedure is to have a special parliamentary committee examine the secret protocols of the government. These protocols disclose the decisions made by the Cabinet concerning military matters and secret diplomatic affairs. The committee reports to Parliament with its approval or disapproval. As a result, according to Oscar Pedersen, Parliament is not only promptly informed as to developments in the conduct of foreign affairs, but it exercises an effective check on the government through the fact that it subjects its policies and acts to examination and revision immediately.

Sweden has a similar commission, but since it reports to the King rather than to the Parliament, it does not secure adequate control.

In most of the new constitutions it is provided that war and peace can be decided only by the legislature. They all have provisions for a legislative commission on foreign affairs similar to the French, but with more powers. The German commission, for example, may sit even when the Diet is not in session. In this manner the executive is subject to supervision, both by the commission and the legislature. The Swiss go a step further by extending the initiative and referendum to treaties, following in this a provision of the French constitution of 1793. For some reason, however, this practice does not seem to commend itself and it is not found in the constitution of the new states. Indeed the Esthonians specifically forbid it.

Into the merits or demerits of the different means of securing more popular control of foreign affairs I cannot enter. In general they are aimed at the time-honored claims of the old school of diplomacy, which holds that questions of a diplomatic nature are too difficult and delicate for legislative supervision or control; that they require speedy action, and that because of their bilateral character popular discussion and vote would cause difficulties with other states. Against this point of view both the theory and the practice, especially in the governments organized since the war years, have made considerable progress.

The principle of democratic control over foreign relations is being more and more recognized. The monarchical control which existed up to 1914 with the more despotic governments is gone entirely. Likewise the extraordinary powers exercised by the executive is being steadily limited by the legislatures. Nor should the fact be overlooked that in many democracies of to-day, the chief of the executive power is himself the direct representative of the people.

Finally there is the League of Nations, whose influence and importance in the matter of foreign affairs is continually underestimated in many quarters. The League got off to a

bad start by being too seriously tied up with the post-war treaties, and the cause of international coöperation has suffered as a consequence. But in spite of this difficulty, which is being gradually corrected, the League represents a great step forward. Here at last the nations of the world have the machinery ready at hand for a reasonable and business-like discussion of their difficulties. The world can no longer be stampeded into a war as in 1914 because of fears and alarms growing out of ignorance or because, from a lack of adequate means for timely discussion, the situation is left entirely to secret diplomacy. At present there are no international problems on which the Secretariat of the League does not already have much data and information. If any acute issue arises as in the Greek-Bulgar dispute in 1925–26, the Council of the League commands not only the machinery for handling the question and the means to set it in motion promptly, but it has ready at hand the needed material on the basis of which it can arrive at a settlement.

Further, by the provisions of the Covenant all treaties between the members of the League to be valid must be registered with the Secretariat. In this way the policies of different nations become fairly well known. Secret commitments unfortunately still exist. Even the terms of important treaties like the French-Belgian accord are not published, the signatories claiming that publicity would destroy the value of the treaty. But the influences against the practice are growing stronger each year. Certainly there is being built up at Geneva a world wide international organization and through it an international attitude of mind, that is exercising a powerful influence on the direction and control of the foreign affaffairs of every nation, whether members of the League or not.

For the present, however, even after we accept the most optimistic claims for progress both through the provisions in the new constitutions, and through the activities of the League of Nations, the fact remains that popular control of foreign affairs still lags far behind the democratization that has taken place in other departments of the governments of

our time; "with all the boasted advance of democracy in other fields," says a well known authority, "a heavy veil is still allowed by mutual consent to fall between the people and the statesmen to whom it is given to open and shut the gates of the temple of Janus." [1] Because of the extreme gravity of the issues involved, however, issues charged with potential tragedy in the life of every nation, this situation is receiving more and more attention and improvements are being made continually. Economic pressure, the need of security against the terrible dangers of scientific warfare of the future and the enlightened opinion of all nations are working for the publicity of international agreements. Diplomacy may have to carry on negotiations in secret, but agreements once reached and signed should be made known not only to the peoples particularly concerned, but to other nations as well. Only in this way can the "Achilles heel of democracy" be made as invulnerable as the rest of the body politic of modern nations.

[1] "Cambridge, History of British Foreign Policy," Vol. III., p. 538.

ADDRESSES FROM SISTER SOCIETIES AND OTHER INSTITUTIONS OF LEARNING

[Presented in the Hall of the Society, April 28, 1927]

To the President and Fellows
of
The American Philosophical Society

held at Philadelphia for promoting Useful Knowledge.

WE, the SENATUS ACADEMICUS of the UNIVERSITY OF ST. ANDREWS, having learned that you are about to celebrate the Two Hundredth Anniversary of the founding of your Society, desire to join the Academies of Learning throughout the world whose representatives will offer their congratulations on the achievements of the past and their good wishes for your future prosperity.

In your founder, BENJAMIN FRANKLIN, an honorary Graduate of our University, we acclaim the author and the statesman, but above all the natural philosopher whose brilliant discoveries in electricity will live for ever. "From this period the electric process passes from the domain of speculative physics into that of cosmical contemplation—from the recesses of the study to the freedom of nature."

We recognise the distinguished services which have been rendered by your Society to the advancement of learning and the development of the arts and sciences.

Our University in celebrating its Fifth Centenary in 1911 did homage to the debt scholarship owes to the United States of America, and, as the oldest University in Scotland, now sends cordial greeting to the oldest Learned Society in America.

The University,
St. Andrews, March 1927.

D. B. Smith.

Vice-Chancellor and Principal.

THE TRUSTEES OF THE UNIVERSITY OF PENNSYLVANIA hereby extend to THE AMERICAN PHILOSOPHICAL SOCIETY HELD AT PHILADELPHIA FOR PROMOTING USEFUL KNOWLEDGE their cordial congratulations and sincere good wishes upon the occasion of the Celebration of the Two Hundredth Anniversary of the Founding of the Society by Benjamin Franklin in the year MDCCXXVII –

Sprung from the same master-brain, the UNIVERSITY OF PENNSYLVANIA pays the homage of respect to the AMERICAN PHILOSOPHICAL SOCIETY, which has completed two centuries of work in promoting Useful Knowledge –

Since the date of its foundation the now venerable American Philosophical Society has held on high the Torch of Truth and the Lamp of Learning; illuminating the dark corners of ignorance and dispelling the shadows of error throughout the world. And so it is the hope of the UNIVERSITY OF PENNSYLVANIA that the AMERICAN PHILOSOPHICAL SOCIETY may, under the grace and prevention of Almighty GOD, continue, through the centuries to come, its beneficent work in spreading Truth amongst all nations, to the end that the fruits of Useful Knowledge may be the rich heritage of generations yet unborn –

Given at Philadelphia in the year of our Lord MCMXXVII and in the year of the University the One Hundred and Eighty-seventh –

Attest:–

SECRETARY

The TRUSTEES of the UNIVERSITY of PENNSYLVANIA –

By

PROVOST

American Academy of Arts and Sciences

On the happy occasion of the
Bicentenary Celebration
of the

American Philosophical Society,

the American Academy of Arts and Sciences has the honor to extend its hearty felicitations. Conscious of the bond which unites the two oldest societies of North America founded for the prosecution of humane and scientific studies, we offer fraternal homage, and express our fervent wish for many centuries of continuous achievement.

Theodore Lyman
PRESIDENT

Robert S. Bigelow
CORRESPONDING SECRETARY

Charles Burton Gulick
RECORDING SECRETARY

Boston, April 20, 1927.

The Massachusetts Historical Society
to
The American Philosophical Society
GREETING:

On this auspicious day, the two-hundredth anniversary of the founding of your association, we recall with satisfaction the fraternal ties of more than a century during which The American Philosophical Society and The Massachusetts Historical Society, in obedience to the precepts of their founding, have striven, each in its own province, to advance knowledge and to foster its application to the benefit of mankind.

Accordingly, we have delegated Lawrence S. Mayo one of our members, to express to your Society our sentiments of congratulation, and to bespeak a continuance of that cordial friendship which we have so long enjoyed.

As we look back with respectful admiration upon your record of honorable service, we cherish the confident hope that the time to come will be marked with even more substantial achievement in the promoting of useful knowledge.

With this in view, there can be no surer guide than the principle that your illustrious founder, Benjamin Franklin, set forth at the very outset of your career and of his own — "the sincere spirit of inquiry after truth, without fondness for dispute or desire of victory".

The Massachusetts Historical Society

Boston, the twenty-seventh day of April, nineteen hundred and twenty-seven.

William Cameron Sheafe Endicott President

Roger Bigelow Merriman
Corresponding Secretary

The Chancellor Vice Chancellor and Governors and the Corporation of McGill University to the Members of the American Philosophical Society held at ... Philadelphia for Promoting Useful Knowledge - Greeting

Learning that you are about to celebrate the two hundredth anniversary of the foundation of your Society and

Knowing that throughout its history your Society has earnestly applied itself to the task then undertaken for the good of humanity

Appreciating the spirit in which you have called to your councils men of other nations and have thus added to your rolls such names as Lavoisier, Linnaeus Priestley and Berzelius

We have appointed Archibald Byron Macallum, M A, M B, Ph D, Sc D, LL D, F R S as our representative to convey to you on our behalf our sincere congratulations upon the completion of the second century of the existence of your Society and to express the hope that its beneficent activities will continue for many years to come

In witness whereof we have caused to be attached hereto the Seal of the University and these presents have been signed on our behalf by the Principal and Vice Chancellor - by the Registrar of the University this twenty-first day of April, one thousand nine hundred and twenty-seven

Principal and Vice Chancellor

Registrar

The Franklin Institute

of the State of Pennsylvania

extends to

THE AMERICAN PHILOSOPHICAL SOCIETY

heartiest congratulations upon the completion of

TWO HUNDRED YEARS

of useful and honorable activity, and wishes sincerely for its associated society an even more brilliant future.

The American Philosophical Society has been from its founding an agency for the promulgation of a broad general interest in Science, and of a wide tolerance in scientific thought. It has proven itself unfailingly antagonistic to an intolerant specialization by the insistence that all subjects presented before its members should be intelligible to the auditors. Through its Proceedings, a wealth of results in investigation and research has been given out to the savants of the world. And it has encouraged and aided the scientists of all nations by the recognitions which it has extended to them for labors successfully done.

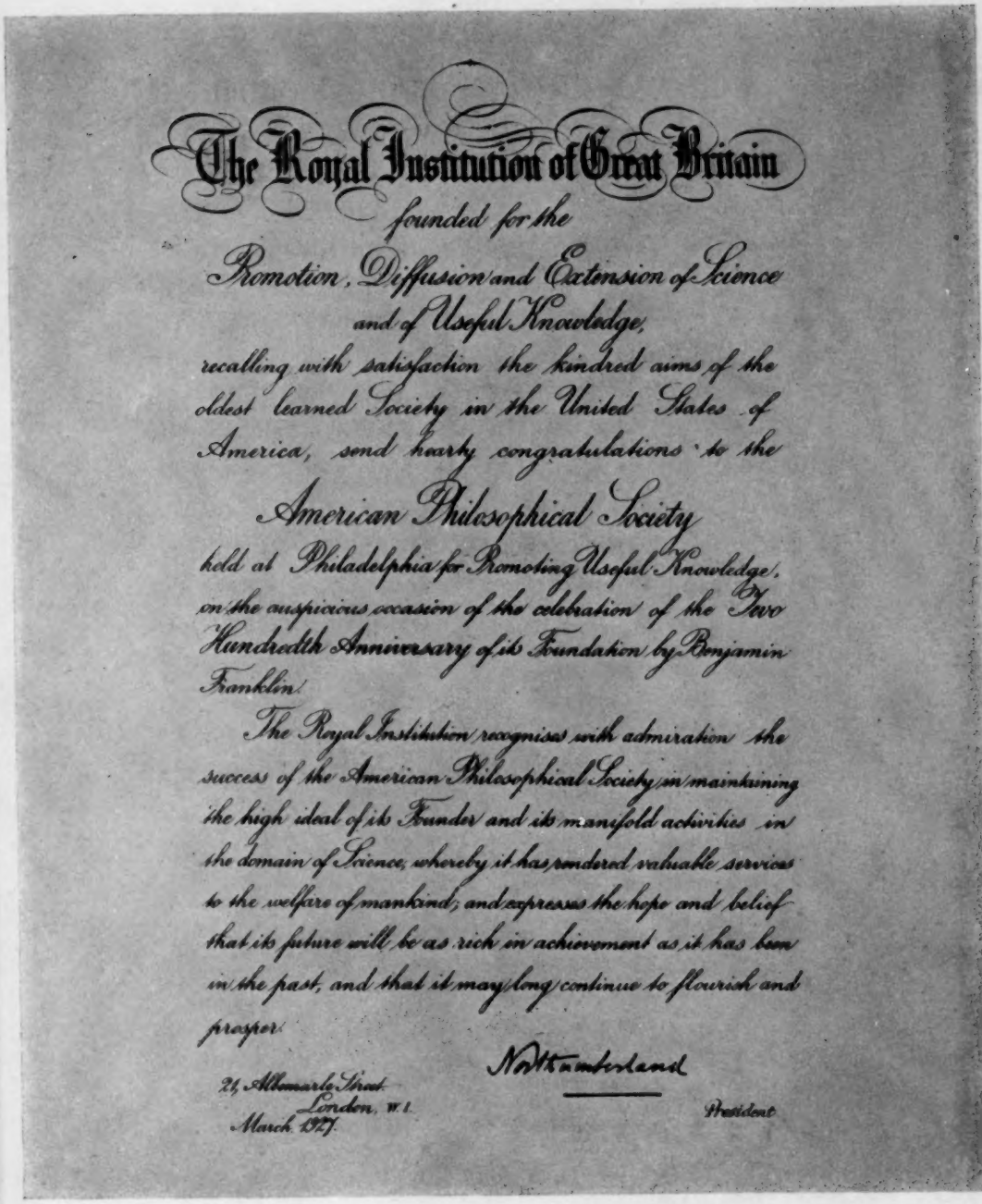
The American Philosophical Society has been an ornament of Philadelphia during well nigh the whole life of the City. It embraces in large measure the scientific aristocracy of America. It is a lamp for light and leading in the realm of scientific thought and action.

For The Franklin Institute of the State of Pennsylvania,

W. B. Kelly
President

Howard McClenahan
Secretary

April twenty-seventh,
Nineteen hundred and twenty-seven.



The Royal Institution of Great Britain

founded for the

Promotion, Diffusion and Extension of Science and of Useful Knowledge,

recalling with satisfaction the kindred aims of the oldest learned Society in the United States of America, send hearty congratulations to the

American Philosophical Society

held at Philadelphia for Promoting Useful Knowledge, on the auspicious occasion of the celebration of the Two Hundredth Anniversary of its Foundation by Benjamin Franklin.

The Royal Institution recognises with admiration the success of the American Philosophical Society in maintaining the high ideal of its Founder and its manifold activities in the domain of Science, whereby it has rendered valuable services to the welfare of mankind; and expresses the hope and belief that its future will be as rich in achievement as it has been in the past, and that it may long continue to flourish and prosper.

Northumberland

President

21, Albemarle Street,
London, W.1.
March 1927.

To The American Philosophical Society

WHEN THE AMERICAN PHILOSOPHICAL SOCIETY IS CELEBRATING THE GRAND MEMORY OF HER FOUNDATION, IN THE YEAR 1727, BY ONE OF THE GREATEST MINDS KNOWN TO HISTORY, BY BENJAMIN FRANKLIN, GREAT IN HIS PATRIOTISM, IN HIS SCIENTIFIC RESEARCH, AND IN HIS WRITINGS, THE SOCIETAS PRO FAUNA ET FLORA FENNICA DESIRES A HUMBLE PLACE IN THAT CONCOURSE OF REVERENTIAL HOMAGE WHICH IN THESE DAYS, SACRED TO MEMORY, FROM NEAR AND FROM AFAR, GATHERS ROUND YOUR SOCIETY.

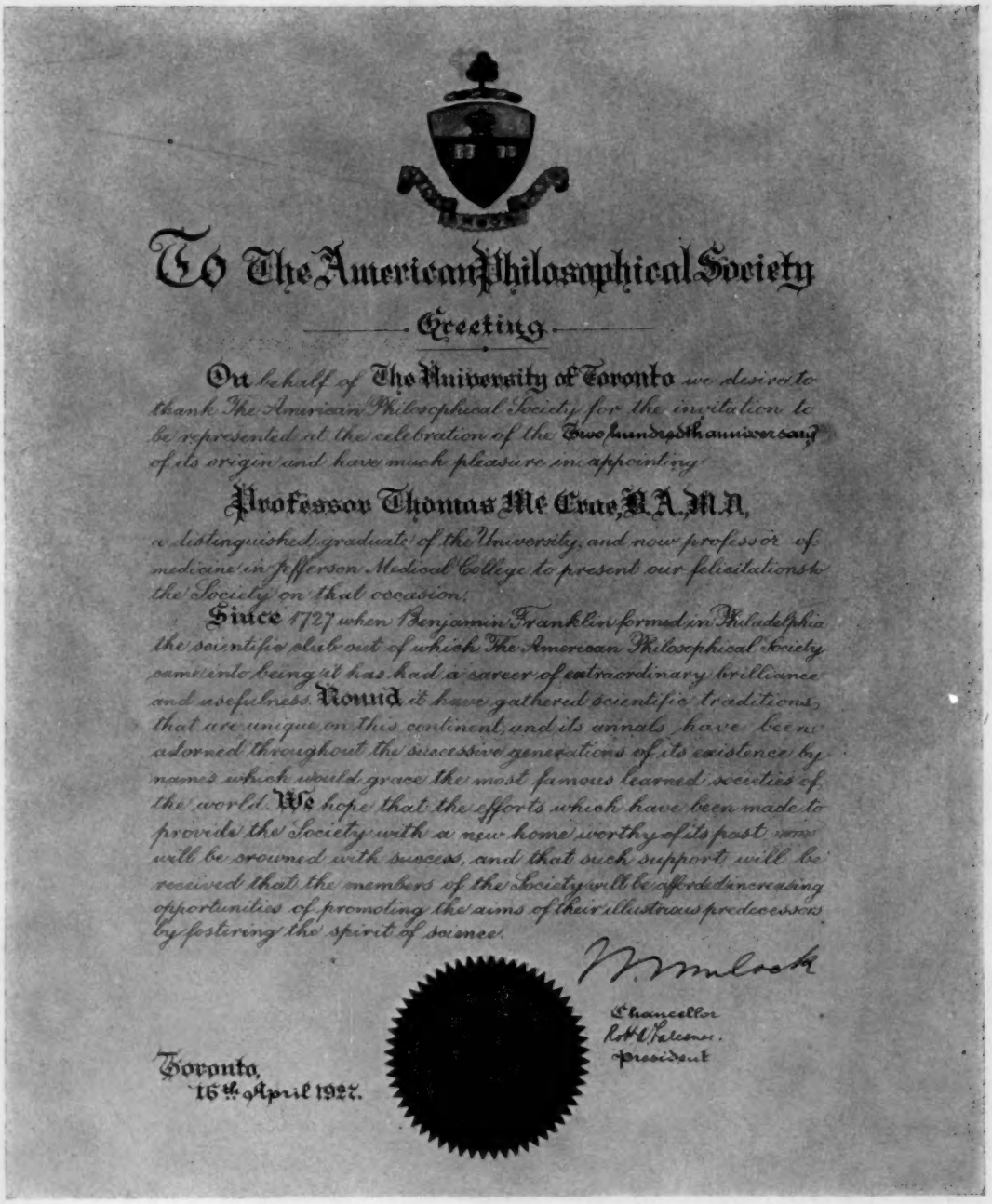
MAY THIS YOUR ANCIENT AND ILLUSTRIOUS SOCIETY IN CENTURIES TO COME PERPETUATE THE NOBLE AND FAR-REACHING IDEALS OF HER FOUNDER, TO THE BENEFIT AND GLORY OF SCIENCE, FATHERLAND, AND MANKIND.

HELSINGFORS, FINLAND 4.4 1927.

SOCIETAS PRO FAUNA ET FLORA FENNICA

Alvar Palmgren — PRESIDENT

Gunnar Ekman — SECRETARY



To The American Philosophical Society

Greeting

On behalf of The University of Toronto we desire to thank The American Philosophical Society for the invitation to be represented at the celebration of the Two hundredth anniversary of its origin and have much pleasure in appointing

Professor Thomas McCrae, B.A., M.D.,

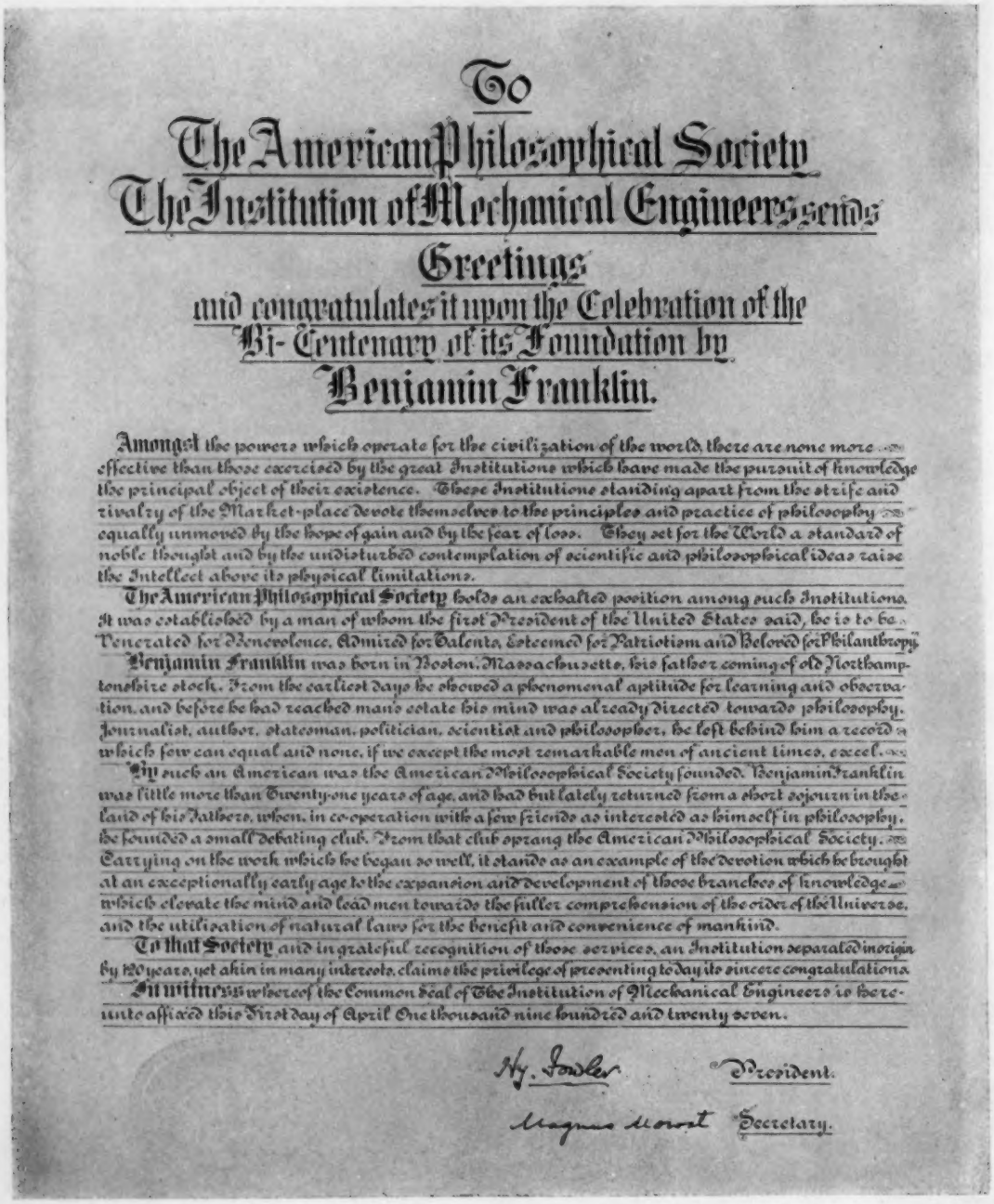
a distinguished graduate of the University, and now professor of medicine in Jefferson Medical College to present our felicitations to the Society on that occasion.

Since 1727 when Benjamin Franklin formed in Philadelphia the scientific club out of which The American Philosophical Society came into being it has had a career of extraordinary brilliance and usefulness. Round it have gathered scientific traditions that are unique on this continent, and its annals have been adorned throughout the successive generations of its existence by names which would grace the most famous learned societies of the world. We hope that the efforts which have been made to provide the Society with a new home worthy of its past now will be crowned with success, and that such support will be received that the members of the Society will be afforded increasing opportunities of promoting the aims of their illustrious predecessors by fostering the spirit of science.

W. H. Mulock

Chancellor
Robt. C. Falconer.
President

Toronto,
16th April 1927.



To
The American Philosophical Society
The Institution of Mechanical Engineers sends
Greetings
and congratulates it upon the Celebration of the
Bi-Centenary of its Foundation by
Benjamin Franklin.

Amongst the powers which operate for the civilization of the world there are none more effective than those exercised by the great Institutions which have made the pursuit of knowledge the principal object of their existence. These Institutions standing apart from the strife and rivalry of the Market-place devote themselves to the principles and practice of philosophy equally unmoved by the hope of gain and by the fear of loss. They set for the World a standard of noble thought and by the undisturbed contemplation of scientific and philosophical ideas raise the Intellect above its physical limitations.

The American Philosophical Society holds an exhalted position among such Institutions. It was established by a man of whom the first President of the United States said, he is to be Venerated for Benevolence, Admired for Talents, Esteemed for Patriotism and Beloved for Philanthropy.

Benjamin Franklin was born in Boston, Massachusetts, his father coming of old Northamptonshire stock. From the earliest days he showed a phenomenal aptitude for learning and observation, and before he had reached man's estate his mind was already directed towards philosophy. Journalist, author, statesman, politician, scientist and philosopher, he left behind him a record which few can equal and none, if we except the most remarkable men of ancient times, excel.

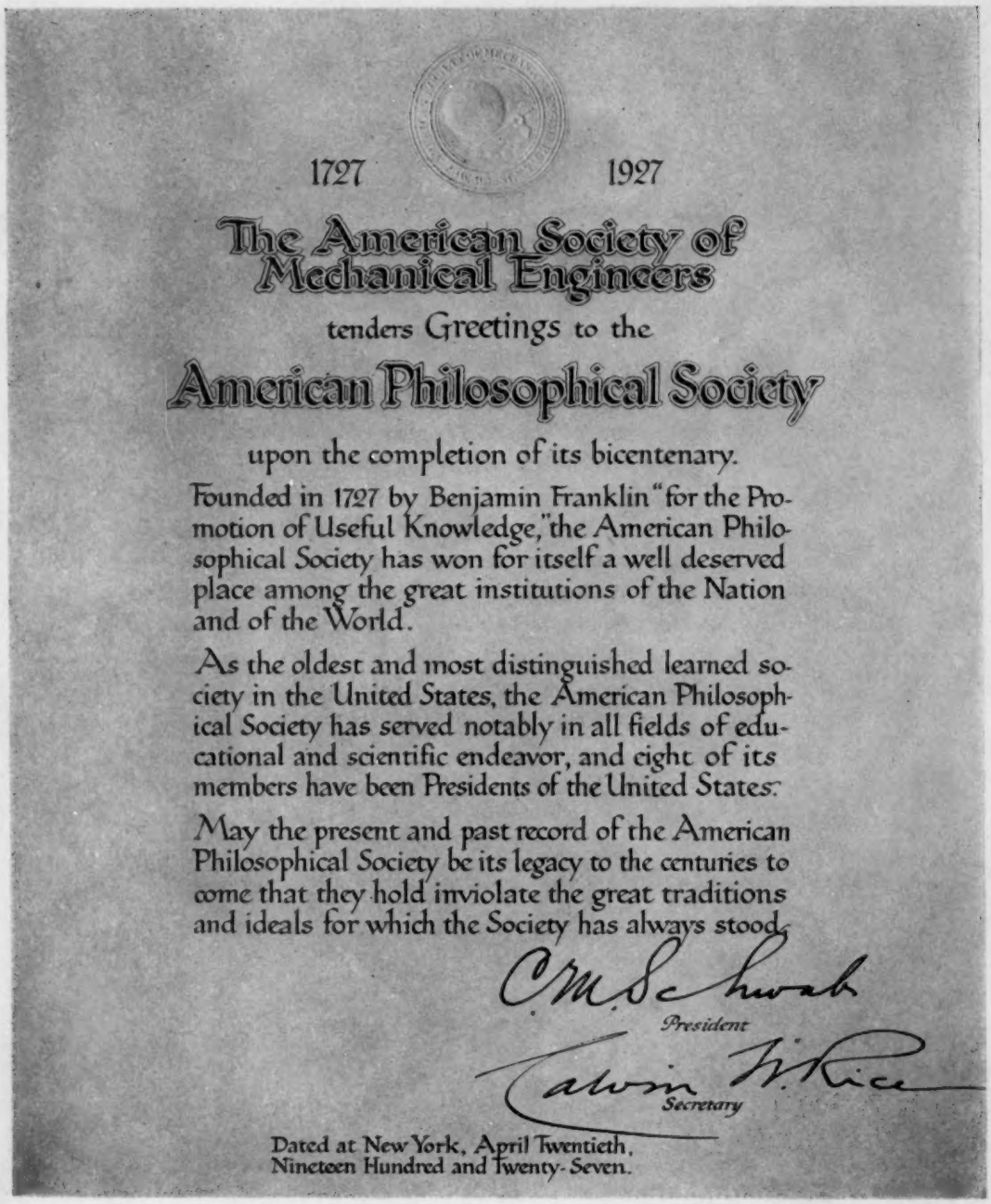
By such an American was the American Philosophical Society founded. Benjamin Franklin was little more than Twenty-one years of age, and had but lately returned from a short sojourn in the land of his fathers, when, in co-operation with a few friends as interested as himself in philosophy, he founded a small debating club. From that club sprang the American Philosophical Society. Carrying on the work which he began so well, it stands as an example of the devotion which he brought at an exceptionally early age to the expansion and development of those branches of knowledge which elevate the mind and lead men towards the fuller comprehension of the order of the Universe, and the utilization of natural laws for the benefit and convenience of mankind.

To that Society and in grateful recognition of those services, an Institution separated in origin by 120 years, yet akin in many interests, claims the privilege of presenting to day its sincere congratulations.

In witness whereof the Common Seal of the Institution of Mechanical Engineers is hereunto affixed this First day of April One thousand nine hundred and twenty seven.

Hy. Fowler President.

Magnus Mowat Secretary.



1727 1927

The American Society of Mechanical Engineers

tenders Greetings to the

American Philosophical Society

upon the completion of its bicentenary.

Founded in 1727 by Benjamin Franklin "for the Promotion of Useful Knowledge," the American Philosophical Society has won for itself a well deserved place among the great institutions of the Nation and of the World.

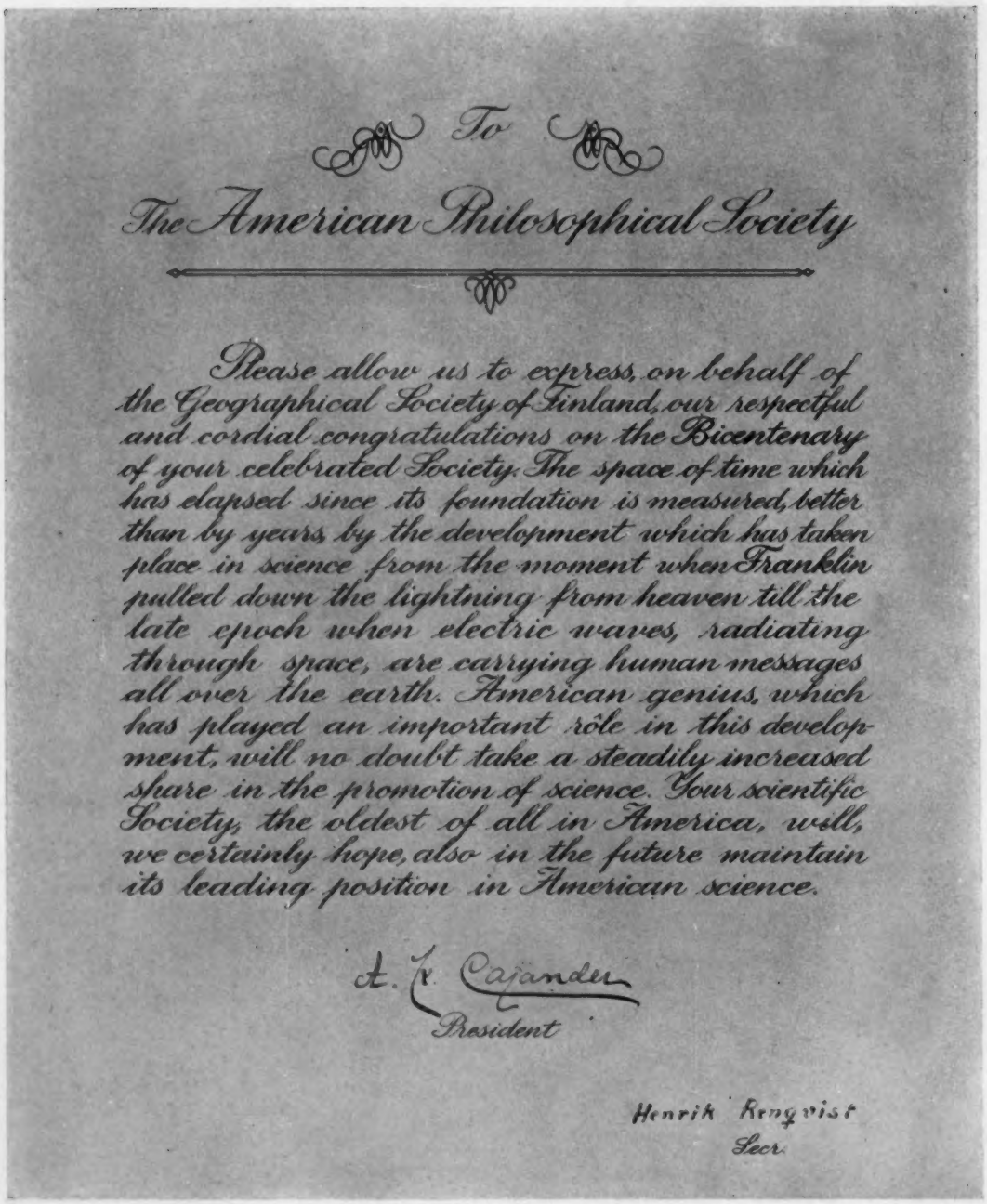
As the oldest and most distinguished learned society in the United States, the American Philosophical Society has served notably in all fields of educational and scientific endeavor, and eight of its members have been Presidents of the United States.

May the present and past record of the American Philosophical Society be its legacy to the centuries to come that they hold inviolate the great traditions and ideals for which the Society has always stood.

C. M. Schwab
President

Calvin W. Rice
Secretary

Dated at New York, April Twentieth,
Nineteen Hundred and Twenty-Seven.



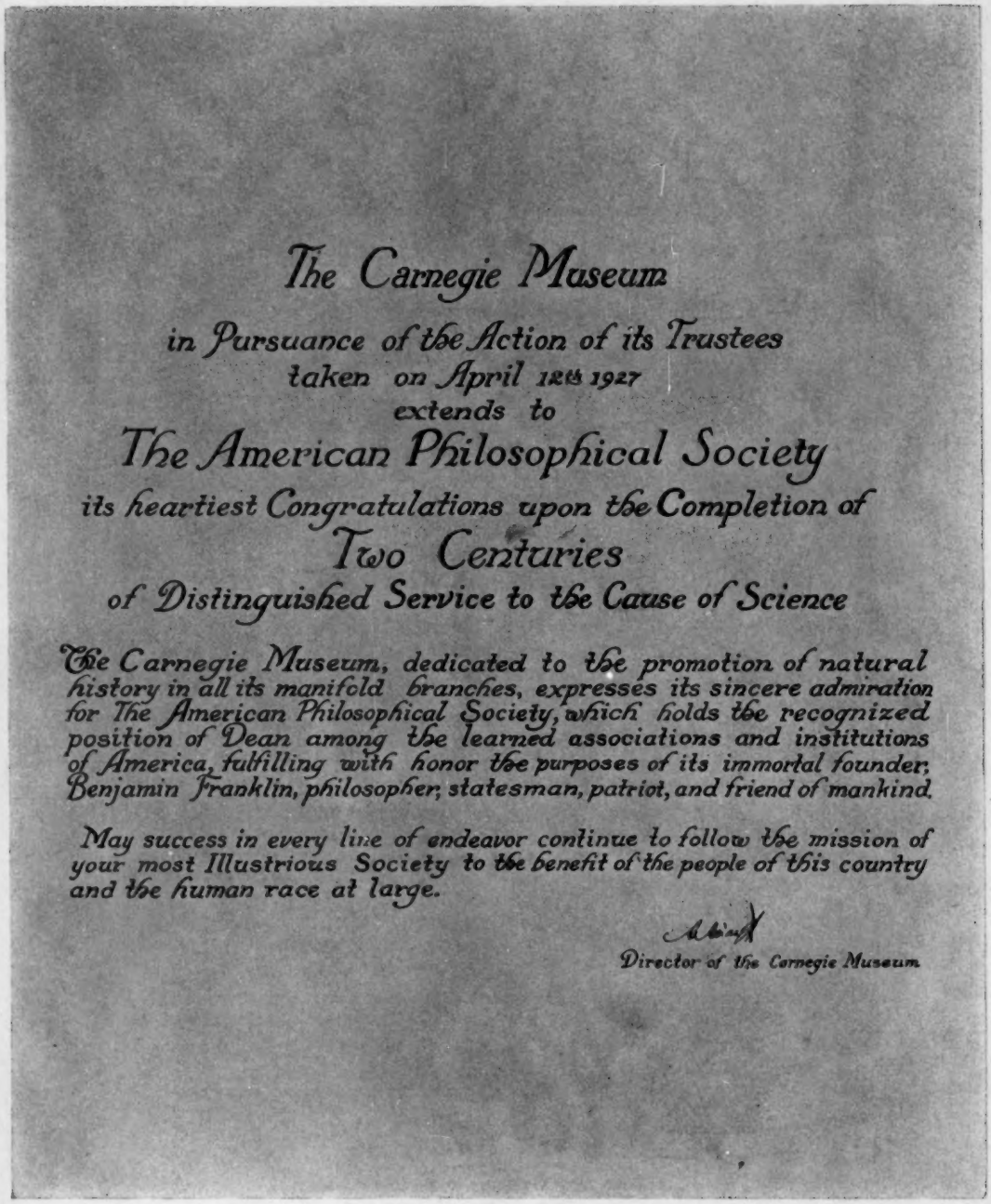
To

The American Philosophical Society

Please allow us to express, on behalf of the Geographical Society of Finland, our respectful and cordial congratulations on the Bicentenary of your celebrated Society. The space of time which has elapsed since its foundation is measured, better than by years, by the development which has taken place in science from the moment when Franklin pulled down the lightning from heaven till the late epoch when electric waves, radiating through space, are carrying human messages all over the earth. American genius, which has played an important rôle in this development, will no doubt take a steadily increased share in the promotion of science. Your scientific Society, the oldest of all in America, will, we certainly hope, also in the future maintain its leading position in American science.

A. K. Cajander
President

Henrik Renqvist
Secr.



The Carnegie Museum

in Pursuance of the Action of its Trustees
taken on April 12th 1927
extends to

The American Philosophical Society

its heartiest Congratulations upon the Completion of

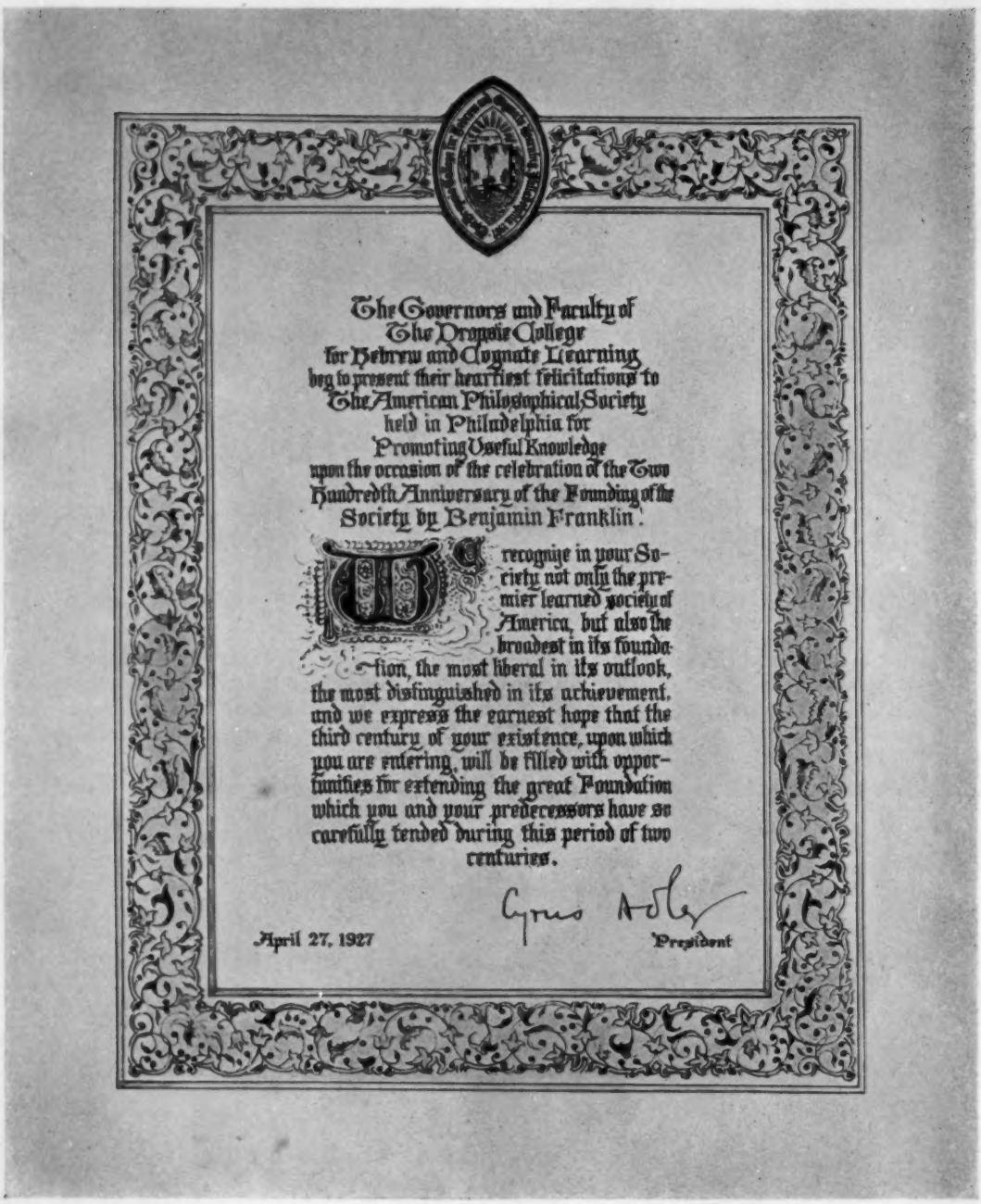
Two Centuries

of Distinguished Service to the Cause of Science

The Carnegie Museum, dedicated to the promotion of natural history in all its manifold branches, expresses its sincere admiration for The American Philosophical Society, which holds the recognized position of Dean among the learned associations and institutions of America, fulfilling with honor the purposes of its immortal founder, Benjamin Franklin, philosopher, statesman, patriot, and friend of mankind.

May success in every line of endeavor continue to follow the mission of your most Illustrious Society to the benefit of the people of this country and the human race at large.

Director of the Carnegie Museum



The Governors and Faculty of
The Dropsie College
for Hebrew and Cognate Learning
beg to present their heartiest felicitations to
The American Philosophical Society
held in Philadelphia for
Promoting Useful Knowledge
upon the occasion of the celebration of the Two
Hundredth Anniversary of the Founding of the
Society by Benjamin Franklin.

We recognize in your Society not only the premier learned society of America, but also the broadest in its foundation, the most liberal in its outlook, the most distinguished in its achievement, and we express the earnest hope that the third century of your existence, upon which you are entering, will be filled with opportunities for extending the great Foundation which you and your predecessors have so carefully tended during this period of two centuries.

Cyrus Adler
President

April 27, 1927

L'UNIVERSITÉ DE PARIS À LA SOCIÉTÉ AMÉRICAINE DE PHILOSOPHIE

L'Université de Paris est heureuse de participer à la célébration du deuxiéme centenaire de votre Société. Deux de ses maîtres éminents vous apportent nos félicitations et nos vœux. Le Recteur et le Conseil, par cette adresse vous expriment toute leur sympathie.

La France tout entiére a gardé fidélement la mémoire de votre fondateur, l'illustre Franklin. Même les petits enfants de chez nous savent qu'il a contribué à fonder l'amitié éternelle de la France et de l'Amérique. Votre République et la nôtre, unies à deux reprises dans deux guerres d'indépendance, unies à toujours dans la paix, se souviendront éternellement de ce grand patriote, de ce grand citoyen de l'Univers, de ce sage, de cet homme de bien.

Nos philosophes n'oublient pas l'illustre fondateur de votre Société. Ils l'honorent comme l'un de leurs prédécesseurs, un des membres de cette Société d'Auteuil où se sont élaborées tant de vues nouvelles et qui dominent la science de notre temps.

L'union d'autrefois est plus vivante que jamais. Il n'y a peut-être pas de pays avec qui la France soit plus étroitement unie qu'avec le vôtre; et grâce à vos philosophes, à vos savants, à vos artistes, et aux nôtres, grâce à vos professeurs et aux nôtres, grâce à vos étudiants et aux nôtres, cette union ne fera que s'affermir.

Votre Société est un ardent foyer de vie spirituelle. Vous suivons de prés ses travaux, nous applaudissons à ses succés.

L'Université de Paris salue votre passé, votre présent et votre avenir.

LE RECTEUR
Président du Conseil de l'Université
S. CHARLÉTY.

Les Professeurs délégués
JULES DRACH
LEÓN ROBIN

(Seal)

(Seal)

BOLOGNA addì 23 Marzo 1927

Queste Autorità Accademiche ed il sottoscritto ringraziano del cortese invito per partecipare alla celebrazione del 2° centenario della Società Americana di Filosofia, che avrà luogo nell'Aprile p. v. e trovandosi nell'impossibilità di inviare una propria rappresentanza questa Università, la più antica del mondo, l'Alma Mater Studiorum, invia la propria fervida ed incondizionata adesione e formula voti perchè la patriottica iniziativa sia coronata dal più completo successo.

Coi sensi di particolare osservanza

Il Rettore
G. IFAMIN

Chiar mo Sigr. Presidente della Società Americana di Filosofia
Filadelfia.

(Seal)

ROMA, 11 Marzo 1927
(anno v)

R. ACCADEMIA NAZIONALE
DEI LINCEI

IL PRESIDENTE
CHIARISSIMO COLLEGA

Nei giorni dal 27 al 30 Aprile p. v. si celebrerà in Filadelfia il 2° centenario della Società Filosofica Americana.

La prego di voler rappresentare l'Accademia dei Lincei alla cerimonia, associandosi gli altri Soci dell'Accademia che saranno presenti, e di recare all'Insigne Società che fu onorata dal nome di B. Franklin il fervido saluto del nostro sodalizio.

Il Presidente
VITO VOLTERRA

Chiar. mo
Signor Prof. Henry Washington
Geophysical Laboratory
Washington, U. S. A.

THE PRESIDENT AND FELLOWS OF HARVARD COLLEGE TO THE AMERICAN PHILOSOPHICAL SOCIETY HELD AT PHILADELPHIA FOR PROMOTING USEFUL KNOWLEDGE

Greeting:

Harvard University sends its congratulations to the American Philosophical Society upon the Celebration of the Two Hundredth Anniversary of the Founding of the Society by Benjamin Franklin, April twenty-seventh to thirtieth, nineteen hundred and twenty-seven, at Philadelphia, Pennsylvania.

Gladly availing themselves of the invitation to be represented at the Ceremonies, the President and Fellows of Harvard College have appointed Edgar Conway Felton, A.B., an Overseer of Harvard College, as their delegate and have charged him to convey their felicitations.

Given at Cambridge on the twenty-first day of March, in the year of Our Lord the nineteen hundred and twenty-seventh and of Harvard College the two hundred and ninety-first.

A. LAWRENCE LOWELL,

(Seal) *President.*

(Seal)

THE PRESIDENT, FELLOWS, AND FACULTY OF YALE UNIVERSITY

take pleasure in accepting the invitation of the American Philosophical Society to be represented at the celebration of the two hundredth anniversary of the founding of the Society, to be held in Philadelphia, April twenty-seventh, twenty-eighth, twenty-ninth, and thirtieth, one thousand nine hundred and twenty-seven.

The University has designated as its representatives James Rowland Angell, Ph.D., Litt.D., LL.D., President of Yale University and Charles Schuchert, LL.D., Professor of Paleontology and Historical Geology, Emeritus, who will convey to you the cordial greetings and best wishes of Yale.

CARL ALBERT LOHMANN,

Acting Secretary.

LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER

INSTITUTED FEB^{Y}, 1781

Societys Rooms, George St.

The Manchester Literary and Philosophical Society sends cordial greetings to the American Philosophical Society on the occasion of the 200th Anniversary of the founding of the Society.

Benjamin Franklin, your illustrious founder, was also associated with the early days of our Society. One of the founders and presidents of the Manchester Literary and Philosophical Society was Dr. Thomas Percival, F.R.S. When Franklin came to Europe on his diplomatic missions, they became friends, and later they corresponded on both scientific and social subjects. One of the consequences of their intimacy was a communication from Franklin to our Society, which was published in Volume 2 of the Transactions. Further, our first Minute Book records that on February 23d, 1785, Benjamin Franklin, LL.D., F.R.S., was elected an Honorary Member of the Society. His name appears in the first list of such members, which also includes the names of Lavoisier, Volta, and Priestly. The name of your founder has therefore been officially connected with out Society for a period of 142 years.

A further link between the two Societies is the exchange of Proceedings and Transactions, which has been maintained for the same number of years. May the friendly connection be long continued.

HERBERT LEVINSTEIN,
President

JOHN ALLAN,
Secretary.

ROYAL PHILOSOPHICAL SOCIETY OF GLASGOW

207 Bath Street

31st March, 1927.

To the American Philosophical Society,
Philadelphia.

The Royal Philosophical Society of Glasgow sends greetings to the American Philosophical Society on the Two Hundredth Anniversary of its Foundation, and desires to express the wish that the celebrations on the occasion may be in every way successful.

The Royal Philosophical Society further most cordially thanks the American Philosophical Society for the invitation to be represented at the celebrations, and regrets exceedingly that it has not been able to arrange for so doing.

Peter Bennett,
Secretary.

DEPARTMENT OF COMMERCE

U. S. Coast and Geodetic Survey
Washington

March 30, 1927.

The American Philosophical Society,
Philadelphia, Pa.

Gentlemen:

I take pleasure in acknowledging your kind invitation to this Bureau to send representatives to the celebration of the two hundredth anniversary of the founding of the Society.

It will not be possible for us to be represented officially, but we shall be deeply interested in the proceedings because of the great significance which the Society, ever since its foundation, has held for the pursuit and progress of scientific research in the United States.

The Coast and Geodetic Survey therefore offers its congratulations to the American Philosophical Society on its long and honorable history, and it hopes for the unbroken continuance of its efforts and influence.

Yours very truly,

R. L. Faris,
Assistant Director.

THE CAMBRIDGE PHILOSOPHICAL SOCIETY

To the
American Philosophical Society
Philadelphia

The Cambridge Philosophical Society sends warmest greetings to the American Philosophical Society on the occasion of the bicentenary of its foundation by the illustrious Benjamin Franklin. It appreciates highly the services which the American Society has rendered in fostering the love and pursuit of science, and the value of the publications which have appeared in its records. The Cambridge Society trusts that its sister Society may flourish in the future as in the past, to a degree worthy of its long and distinguished history.

On behalf and under the authority of the Society
April, 1927.

Horace Lamb, *President*,
F. G. White, *Secretary*,
F. A. Potts, *Treasurer*.

(Seal)

MAGYAR TUDOMÁNYOS AKADÉMIA

Ad 377/1927.

Budapest, 5th April 1927

The American Philosophical Society

The ideas and conceptions of Benjamin Franklin are well known and esteemed in Hungary also, for which reason the Hungarian Academy of Sciences would gladly have availed herself of the opportunity of being represented at the foundation anniversary of your Society.

Unfortunately the financial position of our Society precludes our sending one of our members to America and we have consequently resorted to this means of saluting you on the occasion of the commemoration festivity of the Philosophical Society and of asking you to accept our best wishes for your activity in the future.

We have the honour to remain
Eugen Balogh,
General Secretary
of the Hungarian Academy of Sciences

BOSTON SOCIETY OF NATURAL HISTORY

FOUNDED 1830

No. 234 Berkeley Street
Boston, Mass.

The Boston Society of Natural History accepts with pleasure the kind invitation of the American Philosophical Society to be represented at the celebration of its Two Hundredth Anniversary, April 27, 28, 29, 30, 1927, and takes this occasion to extend its heartiest congratulations on the completion of two centuries of high endeavor as a pioneer in the promotion of science in this hemisphere.

The Boston Society of Natural History has appointed Professor George H. Parker to act as its representative at this celebration.

FRANCIS HARPER,
Secretary.

March 17, 1927.

UNIVERSITY OF MICHIGAN, ANN ARBOR

PRESIDENT'S OFFICE

April 27, 1927

THE SECRETARIES
THE AMERICAN PHILOSOPHICAL SOCIETY
104 South Fifth Street
Philadelphia, Pennsylvania

Gentlemen:

The University of Michigan herewith conveys its congratulations to The American Philosophical Society on the occasion of the Society's Bicentennial Celebration. It felicitates the Society on its long and distinguished career and the place which it has merited in the esteem of the men of science of America. The University of Michigan is happy that its greetings can be conveyed to the Society by the hands of those members of the Michigan Faculty who likewise hold membership in the American Philosophical Society.

Very sincerely yours,

C. O. LITTLE.

THE AMERICAN PHILOSOPHICAL SOCIETY HELD AT PHILADELPHIA FOR PROMOTING USEFUL KNOWLEDGE

Vor zweihundert Jahren hat Benjamin Franklin, dessen Name allen Völkern als forscher und Staatsmann ehrwürdig ist, als einundzwanzigjähriger Jüngling unter der Bezeichnung "Junto" eine kleine geschlossene Gesellschaft begründet, aus der sich binnen wenigen Jahren die American Philosophical Society entwickelte welche als die älteste wissenschaftliche Gesellschaft der neuen Welt, erfüllt von der Jugendkraft des neu aufstrebenden Kulturvolkes als bald eine führende Stellung im geistigen Leben der Vereinigten Staaten einnahm, in der sie sich bis auf den heutigen Tag erhalten hat.

Die Mitglieder dieser Gesellschaft haben aber auch stets den Grundsatz hochgehalten, welchen die Aufnahmsformel des alten Junto den Neueintretenden vorschreibt: "Liebe die Wahrheit um der Wahrheit willen, bemühe Dich sie zu finden, bekenne Dich selbst zu ihr und verbreite sie bei den anderen."

Dies bezeugen die Bände der Proceedings und Transactions, deren erster mit dem Bericht über die Beobachtung des berühmten Venusdurchgangs des Jahres 1769 beginnt un deren stattliche Folge so viele wertvolle Bereicherung in den mannigfaltigen Zweigen der Naturerkenntnis zu Tage gefördert hat.

Eine der jüngeren Akademien der alten Welt, die Akademie der Wissenschaften in Wien entbietet der American Philosophical Society die herzlichsten Glückwünsche zu ihrem Ehrentage. Möge die einer glorreichen Vergangenheit würdige glänzende Gegenwart in, einer verheissungsvollen Zukunft sich fortentwickeln zum Seegen der Wissenschaft, die alle Kulturvölker des Erdkreises vereinigt.

Wien, im April 1927
Die Akademie der Wissenschaften in Wien

Oswald Redlich,
Präsident

Richard Wettstein,
Vizepräsident

Friedrich Becke,
General-Sekretär.

AMERICAN ORIENTAL SOCIETY

FOUNDED 1842

April 26, 1927. 628 West 114th Street
New York, N. Y.

THE SECRETARIES OF THE AMERICAN PHILOSOPHICAL SOCIETY,
104 South Fifth Street,
Philadelphia, Pa.

Gentlemen:

I have the honor to transmit to you the following resolution, which was adopted at the final session of the recent meeting of the American Oriental Society.

"The American Oriental Society, assembled in annual meeting in Cincinnati on April 21, 1927, takes cognizance of the approaching bicentenary of the American Philosophical Society. We desire to record our deep respect for the mother of American learned societies, and to convey to her our congratulations on this notable anniversary. For her future we wish undying usefulness and ever increasing achievement in the causes of learning, in which she has so spendidly led the way."

Very sincerely yours,
CHARLES J. OGDEN,
Corresponding Secretary.

THE PHYSICAL SOCIETY OF LONDON

IMPERIAL COLLEGE OF SCIENCE AND TECHNOLOGY
SOUTH KENSINGTON
LONDON, April 1st. 1927.
S. W. 7.

THE SECRETARIES,
AMERICAN PHILOSOPHICAL SOCIETY.

Dear Sirs:

I am instructed by the Physical Society of London to offer to the American Philosophical Society sincere congratulations on its attainment of the venerable age of two hundred years, and to express on behalf of the Physical Society the confident hope that the American Philosophical Society will continue in future years with renewed vigour to promote useful knowledge, with results even greater than its achievements in the past.

A. O. RANKINE,
Honorary Secretary
Physical Society of London.

(Seal)

THE COLONIAL SOCIETY OF MASSACHUSETTS

TO

THE AMERICAN PHILOSOPHICAL SOCIETY

Greetings:

On the two hundredth anniversary of the first learned society in North America, it is fitting that her juniors express their felicitation and their wishes for many happy returns of the centenary. Although one of the youngest of your fellows, we may claim a unique interest in your celebration, for the Colonial Society of Massachusetts was incorporated for the express purpose of "encouraging individual research into the part taken by our forefathers in the building of our Nation." Benjamin Franklin was one of the most distinguished men born in the Province of the Massachusetts-Bay; and his foundation of the American Philosophical Society in his adopted city of Philadelphia was one of his greatest services to the American Nation and to the Republic of Letters.

We feel greatly honored by your invitation to take part in the exercises celebrating the end of your second, and the commencement of your third centenary; and accordingly, we have delegated our Associate Lawrence Shaw Mayo to offer your Society our sentiments of respect and congratulation.

The Colonial Society of Massachusetts,
SAMUEL ELIOT MORISON, *President*
PERCIVAL MERRITT, *Corresponding Secretary*

Boston, the nineteenth day of April nineteen hundred and twenty-seven

REKTOR UND SENAT DER UNIVERSITÄT FRANKFURT

FRANKFURT AM MAIN, den 6. April 1927

Rektor und Senat der Universität Frankfurt am Main sprechen der um die Wissenschaft hochverdienten American Philosophical Society zu Philadelphia zur Feier ihres zweihundertjährigen Bestehens die herzlichsten Glückwünsche aus zugleich mit den besten Wünschen für ein weiteres Blühen und Gedeihen zum Wohle der Wissenschaft.

(Seal) O. J. GLOFF

The American Philosophical Society,
Philadelphia

CARNEGIE INSTITUTION OF WASHINGTON

WASHINGTON, D. C.

Office of the President

TO THE AMERICAN PHILOSOPHICAL SOCIETY:

Carnegie Institution of Washington sends its cordial greetings to the American Philosophical Society, and begs to state that the bearer, Dr. Charles B. Davenport, Director of the Department of Genetics of the Institution, has been appointed to represent the Institution on the occasion of the celebration of the Two Hundredth Anniversary of the founding of the Society, by Benjamin Franklin, April 27, 28, 29, and 30, 1927.

The Institution takes this occasion to congratulate the Society upon its work and accomplishments and to express best wishes for the future.

JOHN C. MERRIAM, *President*,
Carnegie Institution of Washington

(Seal)

SOCIEDAD CIENTIFICA "ANTONIO ALZATE"

MEXICO

22 de Marzo de 1927,

SR. DR. CHARLES C. HARRISON,
PHILADELPHIA, PA., U. S. A.

Muy estimado Señor:

Habiendo sido invitada esta Sociedad a la conmemoración del Segundo Centenario de la fundación de The American Philosophical Society radicada en esa y aceptada que ha sido dicha invitación, hoy tengo el placer de manifestar a Ud. que esta Sociedad ha tenido a bien designar a Ud. para que se sirva representarlaen la citada conmemoración en unión del Sr. Dr. Edwin S. Balch.

No dudando se sirva Ud. aceptar esta designación me permito anticiparle las mas cumplidas gracias reiterándole las seguridades de mi personal estima.

Soy de Ud. atentamente su afmo. y S. S.

El Secretario perpetuo,
PROF. R. AGUILAR y. S.

SOCIEDAD CIENTIFICA ARGENTINA CEVALLOS 269

BUENOS AIRES, Abril 19 de 1927

SEÑOR PRESIDENTE DE THE AMERICAN PHILOSOPHICAL SOCIETY
PHILADELPHIA.

Tengo el agrado de acusar recibo de su atta. invitación dirigida a esta Sociedad con motivo de la celebración del 2° centenario de esa Institución, cuya atención la Junta Directiva que me honro en presidir agradece muy especialmente a la vez que formula los más expresivos votos por el cumplido éxito de tan importante celebración.

Con tal motivo, pláceme ofrecer al señor Presidente el testimonio de mi distinguida consideración.

H. ROSISIO,
Secretario

EDUARDO HUERGO,
Presidente

(Seal)

BERLIN—LICHTERFELDE
Mommsenstrasse 7/8
d. 20 März, 1927

DER AMERICAN PHILOSOPHICAL SOCIETY

Sage ich für die mir freundlichst zugesandte Einladung für Feier ihres zweihundertjährigen Bestehens den verbindlichsten Dank. Zu meinem Bedaurern ist es mir nicht möglich, derselbe Folge zu leisten. So muss ich mich darauf beschränken, schriftlich meine aufrichtigsten Glückwünsche zu übersenden. Möge ihr in dem neuen Jahrhundert in das sie jetzt eintritt, eine eben so erfolgreiche und segenvolle Werksamkeit beschieden sein, wie sie seit der Zeit Benjamin Franklin's ununterbrochen hat ausüben können!

(Signed) EDUARD MEYER

Wien vu/r Neustiftgasse 12. am 22. März 1927

An die Verehrliche

American Philosophical Society in Philadelphia

Hochgeehrte Herren!

Ihrer freundlichen Einladung zur persönlichen Beteiligung an der Zweihundertjährfeier unserer gesellschaft kann ich zu meinem aufrichtigen Seidwesen nicht nach kommen, weil mich die folgen des Weltkrieges geldarm machten. Sonst aber bin ich als geistiger Arbeiter nach wie vor tätig und benüsse mit freuden den dargebotenen Anlass, um der Gesellschaft meine innigsten Glückwünsche vom ganzen Herzen darenbringen. Ich bin ihr zu ewigem Danke verpflichtet. Als ich noch ein Jüngling i. J. 1883 mich der folkloristik zuwandte, war dieser Wissenszweig bei uns alles eher als anerkannt und die Vertreter der Wissenschaft an der Wiener Universität belegten mich mit Acht und Bann. Damals jedoch würdigten zwei der manschaftesten forscher Albert S. Gatschel und Hoffmann meine Arbeiten als grundlegende Deistungen und auf ihren Antrag hin erwählte mich unsere gesellschaft zu ihrem Korrespondirenden Mitgliede. Diese Ehrung diente mir auf meiner Balkanforschungreise von den Behörden sozusagen als eine Wissenschaftliche Beglaubigung meiner Bemühungen und man behinderte sie nicht allsusehr, was ein grosser Gewinn für mich war. Aus Ihren Proceedings aber, die sie mir seither ständig zuwandten lernte ich gar viel zu und empfieng reichste Anregungen, die mich auf rein nahrwissenschaftlicher Bahn zu arbeiten lehrten und mich von den Irrwegen der Rassentheoretiker abhielten. Ich bin darum unserer gesellschaft zu wahrhaft grösstem Danke verpflichtet, und wenn meine Publikationen mit der Zeit in der weiten Welt Wert und Ansehen gewannen, so ist ein Hauptverdienst davon aufs Konto unserer Gesellschaft zu buchen. Deshalb rufe ich ihr frölich zu:

Vivat, crescat, floreat centenos annos!

Mit herzlichsten Grüssen Ihr Sie
hochverehrender

Prof. Dr. Friedrich S. Krauss

MARCHESE ANTONIO DE GREGORIO

PRINCIPE DI SAN TEODORO

PALERMO, (142) li March, 22, 1927.

Very honored sir

I have had the honor to receive the invitation to take part of the commemoration of the two hundredth anniversary of the American Philosophical Society to which I have the honor to belong.

I am very sorry that I am now in impossibility to leave Palermo and to come in America. So I pray you to show to the Society my acknowledgement of gratitude and thankfulness for the graceful solicitation.

MARQUIS ANTONIO DE GREGORIO

WILHELM OSTWALD

GROSSBOTHEN (SA.)

5. April 1927

DER AMERICAN PHILOSOPHICAL SOCIETY

Beehrt sich der Unterzeichnete, zum hundertjährigen Erinnerungsfest seine ehrerbistigen und herzlichen Glückwünsche zu sagen. Leider ist es ihm wegen seines Alters nicht möglich, der Feier persönlich beizuwohnen und seine Glückwünsche mündlich auszusprechen.

Ergebenst
WILHELM OSTWALD

ADDRESS

By Professor Penck, Berlin

I bring to the American Philosophical Society on the occasion of its second centenary the greetings from the Prussian Academy of Sciences, from the University of Berlin and from the Academy of Sciences at Vienna.

The Prussian Academy of Sciences is somewhat older than the American Philosophical Society. Twenty-five years before the Junto was founded by young men at Philadelphia, it was created by a royal will; and just at that time, when Benjamin Franklin made out of the Junto the American Philosophical Society, Frederick the Great renewed the Berlin Academy. The great king is in some way for Germany what Benjamin Franklin is for the United States. He regarded himself as the first official of the state, and Franklin was called by an American a "king of men. Ay, every inch a king." It is not by chance that both learned societies are influenced by two kings, whose spirit is alive in both, and whose services rendered to commonwealth are reminded to the citizens of two republics by their portraits on the postage-stamps.

The Berlin University is nearly one century younger than the American Philosophical Society. It was founded in a time of great stress, when it was necessary to give to the state spiritual forces instead of the lost material forces. These spiritual forces became very influential during the German wars for independence, and made then out of Germany dismembered in many states a spiritual Great Power. Times of stress again prevail with us. But science continues to flourish, helped by German Emergency Society for Science. As in the Prussian Academy of Science truth is loved for truth's sake in our University; every professor endeavours impartially to find it and to communicate it to his students. Thus the spirit which created the American Philosophical Society is also alive with us. Truth makes independent. The love for truth is intimately connected with the love for independence. To express this feeling just in the neighbourhood of Independence Hall must be for me a high satisfaction.

I forward an address from the Vienna Academy of Science,

written in German. German are the people and the spirit in Austria. Political reasons have drawn boundary lines; but such can never cut asunder one people; and over the political boundary lines which separate different peoples, science is the connecting link of all great nations.

In the feeling, that there is a spiritual community of peoples and nations I bring the best wishes to the American Philosophical Society from the German Empire and from Austria. During two centuries, especially during the last, the American Philosophical Society has witnessed an enormous progress of Science in the United States. I am sure that this progress will continue and that the American Philosophical Society will have in future such an important share in it, as it had until now. The wishes for its future development express only what is my deep personal conviction.

GREETINGS WERE RECEIVED FROM THE FOLLOWING SOCIETIES AND OTHER INSTITUTIONS OF LEARNING

Regia Università Degli Studi, Di Roma 1303
Universität Heidelberg 1386
Universität Wurzburg 1402
Thuringischen Landesuniversität, Jena 1558
L'Académié des Sciences de Paris 1629
Universiteit van Amsterdam 1632
Académié des Sciences Inscriptions et Belles-Lettres de Toulouse 1640
Kungliga Vetenskaps Societen, Upsala 1710
Académié des Sciences, Ussr, Leningrad 1724
Geor. August-Universitat zu Gottingen 1737
Kongelige Danske Videnskabernes Selakab 1742
Gesellschaft der Wissenschaften zu Gottingen 1751
Columbia University 1754
Norwegian Academy of Sciences 1760
Real Académié de Ciencias Y Artes de Barcelona 1763
Bataafsch Genootschap der Proefondervindelijke Wijsbergeetre te Rotterdam 1769
The Royal Gothenburg Society of Arts and Sciences 1778
Linnean Society of London 1788
Botanischer Garten und Botânisches Museum, Berlin 1809
The Academy of Natural Sciences of Philadelphia 1812
Association des Ingenieurs Electriciens Sortis de l'Institut Electro-Technique Montefiore, Liege 1817
Leeds Philosophical and Literary Society 1820
Jardin Botanique de Letat, Bruxelles 1826
Zoological Society of London 1829
Royal Geographical Society, London 1830
University of Missouri 1839
Smithsonian Institution 1846
Real Academia de Ciencias 1847
Royal Meteorological Society 1850
California Academy of Sciences 1853
Koninklijke Academie van Wetenschappen te Amsterdam 1855

Wagner Free Institute of Science....1855
Royal Netherland Academy of Sciences at Amsterdam....1855
University of Chicago....1857
Société D'Anthropologie de Paris....1859
Buffalo Society of Natural Sciences....1861
Cornell University....1867
Davenport Public Museum....1867
University of Illinois....1867
Faculty of Science, Imperial University of Tokyo....1868
Herzlichst Glueckwuensche Berliner Anthropologische, Gesellschaft....1869
Wisconsin Academy of Sciences, Arts and Letters....1870
R. Istituto Botanico di Pavia....1870
University of Cincinnati....1870
Société Fribourgeoise des Sciences Naturelles....1871
Philosophical Society of Washington....1871
Société Geologique de Belgique, Liege....1874
American Chemical Society....1876
Archæological Institute of America....1879
Biological Society of Washington....1880
Tokyo Botanical Society....1882
Société Française Des Electriciens....1883
American Historical Association....1884
Verein für vaterlandische Naturkinde in Wurttemberg....1884
Royal Geographical Society of Australasia Queensland....1885
Societe Belge de Geologie de Paleontologie et D'Hydrologie, Bruxelles....1887
Geological Society of America....1888
École D'Anthropologie....1889
Deutfcher Seefifcherei-Verein, Berlin....1893
Museu Goeldi de Historia Natural e Ethnographia, Para....1894
Sociedad Iberica de Ciencias Naturales....1902
American Phytopathological Society....1909
Universität der Hamburgischen....1919

Foreign Members

Svante A. Arrhenius
Waldemar C. Broegger
Marie S. Curie
Jean A. A. Jusserand
Hendrik A. Lorentz
Fridtjof Nansen
Vito Volterra

TOASTS

SPEECHES DELIVERED AT THE BANQUET HELD
AT THE BELLEVUE-STRATFORD HOTEL,
PHILADELPHIA, PA., SATURDAY
EVENING, APRIL 30, 1927

THE AMERICAN PHILOSOPHICAL SOCIETY

By CYRUS ADLER

(Speech at Banquet on April 30, 1927) [1]

Some years ago, on the first occasion of a visit to the United States of that very charming youth, the Prince of Wales, a great dinner was given in his honor in New York. One of our fellow-members, Mr. Elihu Root, at the conclusion of the feast, read a long scroll to the Prince, in which he extolled the greatness of the British Empire, the virtues of the royal family, and of the Prince himself.

When the Prince, rather disturbed about the correctness of his necktie, arose to reply, he thanked Mr. Root for all the kind things he had said about his country, his family and himself. As for his country, he was sure they were justified; as for his family, he hoped they were justified; as for himself, he would try to live up to them.

The American Philosophical Society would be quite insensible even were it really a soulless corporation, which it is not, if it failed to appreciate the fine things which have been said about it during this celebration. From far and near, we have received cordial congratulations, charmingly expressed and beautifully inscribed. The interest in our past, which the presence of so many distinguished delegates, members and guests testifies, is a source of profound gratification. We thank you for what you have said about us. We, too, recognize the majestic figures that have walked upon our stage. Franklin, Washington and Jefferson; Priestly, Von Humboldt and Rittenhouse; Michaux and Gray; Leidy, Cope and Marsh; the galaxy from the Smithsonian Institution, Henry, Baird, Langley, Goode and Walcott; Henry C.

[1] In response to the toast, "The American Philosophical Society."

Lea, and so many others among the scholars and statesmen of our land, and of the countries beyond the seas, that time would fail if only a tithe of them were enumerated.

We enter upon the third century of our existence, with a measure of self-confidence. We shall try to live up to your expectations of us. There is one guiding principle in our past which we hope contains a seed of promise for the future. This Society has been from its beginning liberal in the truest sense of the word. To be liberal in current parlance generally means to hold that everything that has ever gone before is wrong and that the new theory of the moment is the only one that is right. It is frequently the attitude of "Mary, I fear that nobody but thee and me will be saved, and sometimes I have my doubts about thee."

The American Philosophical Society has had the liberality of hospitality to every kind of thought, to every kind of knowledge, to all the things that form an object of serious study, or promise a useful result therefrom. It was the combination of the ideal and the practical which the spirit of the pioneers and the necessities of the new country all but extorted.

In at least one aspect we are peculiar. It is true that all of the older academies arose before the days of specialization, but most of them yielded to specialization at least to the extent of establishing separate classes or distinct institutes. The American Philosophical Society has never divided into classes but has striven to retain a breadth of interest on the part of its membership.

The world has probably paid for the diffusion of knowledge and for the advantages of the printing press by the development of a larger number of people who know something and a dearth of great minds who can create the synthesis of all the knowledge that is being gathered. The story of the professor who on his death bed lamented that he had never completed his great work on the dative case in Latin and deplored the fact that he had not limited himself to the *dativus ethicus* represents the kind of specialization which has passed for knowledge.

I would not have you think, however, that this Society is to-night in an unhappy or critical mood. Far from it. We rejoice at all the avenues for research and publication which have been opened up since the two hundred years back when Franklin formed, as he put it, "most of my ingenious acquaintance into a club for mutual improvement" wherein "Morals, Politics or Natural Philosophy" might be discussed.

In the nations of the earth learned academies have grown and multiplied. All departments of knowledge have succeeded in bringing together their votaries into innumerable societies. The universities and colleges—once merely associations of masters and teachers for the imparting of ascertained knowledge have become important centers of research and publication. The museums, originally cabinets to hold rare and curious objects, later developing into vast national storehouses of objects of art and science acquired by loot or otherwise, have, by the very force of the accumulation of material, become centers of research. The libraries, once mere depositories of manuscripts and books, rather to be kept in custody than used, have through the sheer weight of the opportunity afforded by the material gathered likewise become centers of learning and publication.

While all this was going on abroad—the American Continent, engaged in the fierce struggle inherent in the conquest of nature by man, developed innumerable academies, societies, museums and libraries in every State of the American Union, at the National Capital and in all of the countries of Central and South America, as well as among our British neighbors of the North.

Added to these more recently have been the great Foundations, either general or specific, which have created new possibilities of research never before dreamed of.

In the face of this growth and change, what should be the policy of the American Philosophical Society, entering upon its third century? Its past is secure. Its future will, no doubt, take care of itself. The present is our concern.

What course shall we take? I believe that there will be no dissent from the proposition that we maintain our old attitude of liberality. Here astronomers and physicists, chemists and geologists, biologists and paleontologists must always be welcomed. Alongside of them the historian and the anthropologist, the orientalist and the classical scholar must be found. Here the statesman and the jurist, the engineer and the inventor, the physician and the economist must have a home.

But how can they live in this home? It has grown inadequate. The charming structure granted to us by the State of Pennsylvania in 1785 is no longer sufficient for our needs. And limited in space as it is, we cannot occupy even the whole of it, for the lower floors must be used for income producing purposes. Stored in our vaults in that building and elsewhere there are treasures of manuscripts and objects which would make any museum or library famous. But mark you, I said they were stored and that is the literal truth. They are not exhibited; they are hardly available. If five students were to undertake to work in our rooms at one time, we would be overcrowded.

It is true that years ago, the City of Philadelphia offered our Society a beautiful site on the new Parkway, but either through lack of will or lack of support, this offer has not been availed of.

Possibly both reasons played a part. It is more than likely that the "Philosophical Society urge" has not been strong. That the situation on Independence Square—one of the most beautiful and historic spots of America—has enchained us. That we cannot bear to give up the delightful atmosphere crowded with the memories of Franklin, Rittenhouse and Jefferson. So be it—though let me say in passing that even if we decide to retain the historic and artistic atmosphere, the physical atmosphere of our Hall sometimes reaches the asphyxiation point.

"How long halt ye between two opinions." One of two steps should be taken. Either a sufficient fund should be se-

cured to erect a new building or one should be obtained to remodel the present building so that it should be adequate and safe. Either alternative requires a considerable fund. If the corner of 5th and Chestnut Streets should go up in flames, more of American History, more of the History of American Science would perish than in any space on this continent fifty times its size. Can the Society or its members or the people of America permit a condition such as this to continue?

But it is more than mere safety that we should expect. These treasures are here to be used for the advancement of knowledge. A Society which is entrusted with these priceless objects should also be entrusted with the means for their study and publication. This, in spite of all the wealth that America has gained, has never been accorded to the American Philosophical Society. Why not? I wonder. Is it an exemplification of the Hindu proverb "He who begs in silence, starves in silence"?

Or has our direction and policy been considered unworthy? I shall not hark back to the supermen of the beginning of this Republic. Let us turn to a recent period. At this table are seated two men who guided our destinies for many years, Doctor W. W. Keen, the nestor of American surgery, and the distinguished palæontologist, Professor William B. Scott, himself a descendant of Franklin. Who of us can forget the great vision of Doctor William Pepper? It is not that we lacked leadership. The fault, dear Brutus, is not in our stars but in ourselves. We have appointed leaders, but have not supported them. We have at this, our first meeting of the third century, elected a new President, Doctor Francis X. Dercum, worthy of the long line of his distinguished predecessors. Let us pledge him our real, active, whole-hearted support, not solely as a matter of personal loyalty, not even to aggrandize the fame of our Society, but to the higher end that useful knowledge should always be promoted from this the primal seat of research in our country, that for all time those who are looking for the light shall find as a beacon the torch of Franklin—The American Philosophical Society.

OUR UNIVERSITIES

By EDGAR ODELL LOVETT

(Speech at Banquet on April 30, 1927)

Mr. Toastmaster, Ladies and Gentlemen:

Writing from Philadelphia, May 16, 1775, the Founder of this Society and of the University of Pennsylvania says: "I had a passage of six weeks; the weather constantly so moderate that a London wherry might have accompanied us all the way. I got home in the evening, and the next morning was unanimously chosen by the Assembly a delegate to the Congress now sitting." Writing from New York on March 28, 1790, the Father of the University of Virginia, for many years President of this Society, says: "I arrived here after as laborious a journey of a fortnight from Richmond as I ever went through . . . My first object was to look out a house in the Broadway if possible, as being the centre of my business. Finding none there vacant for the present, I have taken one in Maiden Lane, which may give me time to look about me."

On these long journeys Mr. Franklin arrived at, and Mr. Jefferson set out from, the western frontier of their country. In the intervening years that western frontier has crossed the Alleghenies, crossed the Mississippi, crossed the Great Plains, crossed the Rockies, reached the Pacific, started back again as an eastern frontier to reënlighten the East from the West, has reached Houston, Texas, and is on its way, we trust, to the more or less provincial places of the Atlantic seaboard. The western frontier carried universities with it straight across the continent, and the eastern frontier is bringing universities with it straight across the continent. Not only in point of frontiers, but also with respect to

duration of time, the journey that brings me to this meeting has advantage over those of Mr. Franklin and Mr. Jefferson just mentioned, for day before yesterday I did a day's work in Houston, Texas, and to-night, believe me, I am doing a day's work in Philadelphia, Pennsylvania.

Joel Chandler Harris, author of the Uncle Remus, Br'er Rabbit, and Tar-Baby stories, was at his desk in the office of the *Atlanta Constitution* one night when an old-time reporter looked over and said:

"Joe, how do you spell 'graphic'? With one 'f' or two?"

"Well," said Joe, who was too kind-hearted to wound the pride of inquiring ignorance, and too gentle to harm even a common adjective, "if you are going to use any, Bill, I reckon you might as well go the limit."

Definite limits both of time and of subject have been put upon the speakers this evening. These limits I propose to respect. Like all subjects worth speaking to, the toast assigned to me is a large one, always timely, and sufficiently solemn, for there are universities and universities and universities in this country. In the few and brief remarks I am about to make I have sought out a common denominator or two, and I offer them in the spirit of the presiding officer of another assembly, who said that he proposed to conduct the proceedings of the meeting in such a way as to avoid partiality on the one hand and impartiality on the other.

The Idea of a University

In the shortest possible terms, these universities are simply communities—communities of men, communities of women, communities of men and women, in search of knowledge, and to that end living a common life. It was in mediæval times that modern universities began, and in Latin, the standard language of the time, the word *universitas* meant precisely what we now mean when we use the word corporation, for it meant literally "the whole of us." The university was a corporation existing under several different forms, as, for example, a corporation of schools or colleges,

a corporation of masters and teachers, or a corporation of students and scholars. The present business of such a corporation as a university is to think and to teach; to track the trail of truth; to transform mind into a higher order of mind, and to train men and women of thought and men and women of action. Its raw materials are the children, all the children of all the people, and the problems of the civilization in which they live. Its processes are those of inquiry, instruction, test, and experiment. Its products are citizens and scholars, science and learning. Its dividends are paid in the discipline of soul that, through righteousness, justice, and truth, exalteth men and exalteth nations.

An Influence of the University

Thus might be framed the idea of a university. Now an immediate influence of the idea appears in an increasingly widespread diffusion of educated mind among our people, and there are certain earmarks by which the educated mind may be recognized. In virtue of the business in which I happen to be engaged, I have an acquired prejudice for the university mind, yet some of the best of educated minds I have known never had academic associations either early or late. We might all agree, I think, that the first and finest thing about the educated mind is that it is not fixed and final, but fluid and forming, constantly in the flow of change; not closed, but open, with the eager inquisitiveness and insatiable curiosity of a child; impetuous, impulsive, adventurous as youth, yet held in leash; deliberate but decisive as the mind of affairs; conservative and cautious as the mind of age. This characterization, to be sure, is rather a large order, but there are certainly such consistent attributes as independence and judgment; sincerity, with magnanimity; impartiality, with sympathy; tolerance, if sometimes ironical; temperateness and moderation; and a nice sense of fitness even to the point of artistry. To me a very striking thing about the educated mind to-day is that almost without exception it begins as an individual mind and ends as an

international mind. Such international mind thinks in terms of the planet, all the people of the planet, all the people that were and are and are to be. It seeks to survey the whole of time and existence in terms of civilizations and their history. It is among men a very recent notion. It has been given currency in our generation by publicists at home and abroad. It implies the national mind, even as the national mind implies the provincial mind, but it would transform patriotism into a passion for humankind. It sees mankind the world over as one great family, its dominating idea being the essential unity of civilization. Its power is persuasion. Its appeal is to reason. Its purpose is harmony and peace, among men and among nations.

Whether we will it or not, the world in our day seems destined to come more and more under the dominion of some form or other of international mind, under a cosmopolitan spirit that shall mould, and in turn be moulded of, world opinion. Of such mind the religion of Christendom was a pioneer; to its cosmopolitan spirit science and art have made universal appeal; towards its cosmopolitan comity trade in goods and commerce in ideas have contributed; towards its international polity the higher patriotism has always pointed. This planetary aspect of international mind doubly enhances the power of all educational enterprise, not only to educate the public opinion of a people, but to enlighten the public opinion of a planet on which no people longer lives alone.

Abiding Character of Idea and Influence

Finally, the university and its idea, the idea and its influence, abide in the personality of the university. To Mr. Franklin and to Mr. Jefferson the most wondrous sight they ever saw was the university. To you and to me, in the most impressionable of the formative years of our life, next to the Founder of your University, next to the Father of my University, the university itself was the outstanding personality. It was literally a living presence, and it spoke

to us. From the past and for the future it spoke to us. Through history and philosophy and science, it spoke to us of truth and justice and freedom. And its voice was a very human voice. The manifold manifestations and ministrations of its spirit, in its freedom for untrammeled search for truth and disinterested service to State and Church, won alike our admiration, our zeal, our loyalty, and our affection. In its own life the university had attained to greatness of living. It called to us, and insistently, for the intelligence, energy, courage, enthusiasm, and independence that characterize all greatness of living, whether of individuals or of institutions. Such greatness of living your university and my university had achieved through fire and pestilence and poverty and war. Independently of its individual members, each had come into a kind of immortality of its own, for, as has frequently been remarked, the university in all ages has proved to be about the most enduring of human institutions. Think of the changes in Church and State which have been witnessed and weathered by universities the world over. For this survival there is a reason, for so long as men love learning, so long will the university flourish. So long as men seek truth, even so long will the spirit and service of science endure. And in beauty and holiness, religion and art will outlast them all.

I therefore confidently anticipate, that when our successors sit in conclave at the next centenary celebration of the American Philosophical Society, they will find our universities resolutely carrying the great cause of science and learning forward to still higher conquests, and in ways altogether worthy of our illustrious predecessors, whom we hold in grateful and appreciative remembrance to-night.

BENJAMIN FRANKLIN

By RUSSELL DUANE

(Speech at Banquet on April 30, 1927)

On the title page of John Bigelow's edition of Franklin's Works appears the celebrated quotation from the *Iliad*, "Strange that Ulysses does a thousand things so well." What was the cord which bound together these marvellous pearls of achievement? In seeking to answer this question it has seemed to me that the keynote of Franklin's life might well be described as his "dynamic spirit of progress." He was possessed with a boundless spirit of inquiry. He was forever seeking after a better way. It was his constant aim first to find the truth and then to utilize it in the service of humanity!

After the great philosopher and inventor Farraday had given a private exhibition of his new invention, the electro-magnet, it is related that Mr. Gladstone said to him, "This thing is most interesting, Professor, but of what use is it?" whereupon Farraday replied, "Well, perhaps some day you may be able to tax it." To Franklin the question was once propounded "Of what use is electricity?" He replied, "Of what use is a baby?"

In the field of practical affairs it was Franklin's belief that there were few human undertakings in his day which could not be carried forward by greatly improved processes. Early in life, as a merchant, he developed new forms of salesmanship and advertising. He originated the idea of the department store. As Postmaster of Philadelphia and later as Postmaster-General of the Colonies, he introduced speedier transportation and other new methods into the postal service. During the intervals of his diplomatic duties in London he

interested himself in such matters as phonetic spelling, a reformed alphabet and improved methods of ventilation. He experimented for the purpose of finding a cure for colds which he attributed largely to bad air and overfeeding. He made other experiments to produce a stove which would burn its own smoke and a grate which would send its heat into the room instead of up the chimney. In his military campaigns against the Indians in the interior of Pennsylvania, in which he rose to the rank of Colonel of the provincial militia, he showed his ingenuity in the construction of forts, and at the opening of the Revolutionary War he invented ingenious forms of *chevaux-de-frise* to block the river Delaware against the British fleet. These activities were the outward expression of an all-enquiring, progressive mind. Whether engaged in a study of the gulf stream or the origin of northeast storms or the composition of lightning, he was forever seeking out causes, analyzing effects and devising remedies. I recall that at a previous annual banquet of this Society the late Horace Howard Furness, in responding to this same toast, said that in discovering the cause and nature of thunder storms Franklin had "destroyed one of the greatest sources of human terror!"

Franklin was also a pioneer in the field of ethics. His act in founding the Junto in 1727—which marked the inception of our Society and gave us the occasion for this present Bicentennial Celebration—was largely prompted by his desire to promote among his fellow townsmen progress in the art of right living. His brief rule of conduct was "to do good and live a satisfying life." Hence the importance which he attached to the virtue of "thrift," on which subject he gave us such striking maxims. It was this instinct which led him to incorporate in his will his celebrated bequests to the town of Boston and to the city of Philadelphia, to be compounded in part for 100 years and in part for 200 years and then to be expended in public improvements. The great trade school in Boston known as The Franklin Union, of which my father was one of the organizers and trustees,

was founded at the end of the first century as the result of Franklin's foresight in directing these accumulations of his money. The fund which he left to the city of Philadelphia has gone to promote the development of education at the University of Pennsylvania, of which Franklin was one of the founders. It is appropriate that his estate should be thus expended, because in no field of human activity was his progressive spirit so strikingly manifested as in the field of education. When I was a student at Harvard College methods of teaching English composition were in vogue which were supposed to be novel, original and effective, consisting of the writing of daily themes and the paraphrasing of the works of great authors, followed by a comparison with the original. At the time I assumed that these methods represented the very latest experiments in Harvard culture, but there was not a single one of them which I did not afterwards find exploited in detail in Franklin's writings on the subject of education. In dedicating a statue to him many years ago, I referred in some detail to Franklin's writings on education and especially to his prospectus of the new college later known as the University of Pennsylvania, and said:

"Franklin broke away from the conventional system of education at a hundred different points. He reduced in rank the study of Latin and Greek, he raised that of French and German. He introduced advanced instruction in the English language, and the study of English literature, composition, logic and oratory. He was the first to assign to their true place in a system of college education the subjects of history, political economy and the science of government. He forestalled the modern system of elective studies, and prescribed alternate courses of instruction which should be adapted to the needs of every student, and which should serve as a preparation for his future vocation in life. Franklin's system of education embraced also the principles which underlie all of our modern schools for technical training in the arts and sciences. He believed that college students should not only read books, but that they should study maps, prints and

designs, become familiar with the use of machines and instruments, and learn to conduct experiments in the domain of natural philosophy and mechanics. He prescribed technical instruction in agriculture, in mathematics, in accounts, in drawing, and in architecture. The great work which is being accomplished to-day in our Wharton School of Finance, in our mechanical and chemical laboratories, and in a dozen other departments of our University, are but the more complete fulfillment of Franklin's system of technical education. And lastly, Franklin was the first educator of modern times to prescribe a regular system of athletic training as an indispensable feature of college life."

Franklin was equally original and progressive in his ideas on the subject of *Primary* Education. In his "Sketch of an English School" he favored the use of spelling bees for young children, the practice of reading aloud, the study of elocution, the opportunity to students to do public speaking before their classmates, and the constant writing of short papers on what he called "common occurrences." As I have already said, he was the inventor of the "Elective System" of studies now universally adopted in our colleges. He even invented the so-called "Group System" which many educators regard as the very latest thing in advanced education. Very little can be found in the evolution of education as a science in our day which Franklin did not originally suggest. He anticipated Adam Smith in the importance which he attached to Political Economy and what we now call "Political Science." Just as he believed in the study of hygiene for our bodies, so he believed in a sound and progressive kind of education to promote our mentality. He believed that morality, good manners, patriotism and good citizenship could be learned through a study of the biographies of the great men of the world. These conceptions, believed by so many to have originated in our day, are all to be found in Franklin's writings.

Franklin was a Progressive in the field of Religion. Having much time on his hands during his diplomatic mission

to London between 1765 and 1775, he diverted himself by trying to reform the Church of England. In conjunction with one of his friends in the British Peerage he prepared a revision of the English Prayer Book to which he wrote an eloquent preface. In this revised work he cut down the catechism to two questions, "What is your duty to God?" and "What is your duty to your neighbor?" He excised large portions of the Psalms because, as he said, "they imprecate in the most bitter terms the vengeance of God upon our adversaries contrary to the spirit of Christianity which commands us to love our enemies and to pray for those that hate us and despitefully use us." In his preface to this revised prayer book he struck a very modern note when he said that his object in writing it was "a humanitarian one—to prevent the old and faithful from freezing to death through long sermons in cold churches, to make the services so short as to attract the young, and to relieve the well-disposed from the affliction of interminable prayers." He was a militant foe of religious intolerance, as shown by his attitude toward certain of the clergy in Pennsylvania who persisted in praying for the King. Some hot-headed young fellows came to Franklin one day and complained that these prayers, if allowed to continue, might seriously jeopardize the result of the Revolutionary War. Franklin quieted their apprehensions by saying, "Don't be disturbed about those men. They won't do any harm. They have a very small interest up in the court of Heaven. To my personal knowledge they have been praying for at least 15 years that the Lord would endow King George and his ministers with wisdom, and there is not the slightest evidence that any attention whatever has been paid to their petitions."

Franklin was always a Progressive in politics. He was the first to visualize in any practical way the union of the Colonies, when he proposed at the Congress of Albany in 1754 the first plan for joint action by the Colonies in defense of their interests. The "plan of union" proposed by him after his return from Great Britain in July 1775, closely

anticipated in its structure and provisions the present Constitution of the United States. He had a genius for expressing political truths in new and striking ways which would command attention. In so doing he often employed his great gift of wit as, for example, in his celebrated essay on "The Rules for Reducing a Great Empire to a Small One." His elasticity of mind was such as to give him a lightning-like power of progressive change. It is hard to realize that only four years elapsed between his appearance before the Privy Council of Great Britain, at a time when he was striving to reconcile the Mother Country to the Colonies, and that day in Paris, February 6, 1778, when he signed in behalf of the United States the Treaty of Alliance and the concurrent Treaty of Amity and Commerce with France. It is said that, recalling the abuse heaped upon him by certain members of the Privy Council on the earlier occasion, he wore the same velvet suit when signing these Treaties, and it is also said that he wore this same suit on that still later day in 1783 when he signed the Treaty of Peace with Great Britain which effected the final rupture with the Colonies and brought the United States of America into the concourse of nations.

It was said by General Washington that without Franklin's successful efforts in securing the aid of the French, the Revolutionary War would have failed. The diplomacy by which he accomplished this result was as original and progressive as his other achievements. He was probably the first diplomat in history to practice the modern theory of diplomacy, according to which an Ambassador should seek to accomplish his results, not merely by contact with the Government of the country to which he is accredited, but by influence exerted through personal friendship with its leading men and women. The alliance between the Colonists and the French and the coöperation of their military and naval forces which finally terminated the Revolutionary War, were largely brought about through Franklin's personal friendship with the prominent men of France and also by

the personal attachment which he inspired in so many of the women of the French nobility. It was a personal triumph which has rarely been equalled in the history of diplomacy. One of his biographers said of him in this connection, "Envoys like John Adams attended strictly to business and failed in their missions; Franklin neglected business—at judicious intervals—and success emptied its largest cornucopias over his head." So keenly did King Louis XVI appreciate him that he once remarked, "Whenever Dr. Franklin arrives in Paris I become the second man in my own dominions."

Such achievements certainly justify my theory that Franklin was inspired by "a dynamic spirit of progress." In fact it might almost be said that his personality resembled that evolutionary force which throughout the ages has developed nature itself and natural law as they now exist. And hence it is that posterity has awarded to him his lofty place among the great pioneers of all time, the guides to right living, the creators of great foundations, the proponents of progress in the arts and sciences, the apostles of political and religious freedom, the prophets of new dispensations and the founders of great states.

OUR SISTER SOCIETIES

By JOHN C. MERRIAM

(Speech at Banquet on April 30, 1927)

The American Philosophical Society occupies a position of unusual interest among organizations of America devoted to development and interpretation of knowledge. The linking of those aspects of science which touch accumulation and interpretation of facts to the group of problems concerning human interests is one of the characteristic features of the Society. This agency is concerned not alone with ideas, which in another form of expression may be called knowledge. Its organization and general programme represent also the statement and interpretation of ideals, which may be described also as wisdom.

From the time of its origin, the Philosophical Society has concerned itself with the extremely interesting problem of finding human application of science and, at the same time, attempting to obtain scientific foundation for study of characteristics peculiar to man. It attempts in its organization to bring these various subjects into their true relation to each other. The Society recognizes that safety of mankind lies not alone in discovery of impersonal laws and attempts to secure their enforcement. It realizes that the ultimate goal in human organization must be attainment of a type of relation in which law-understanding and acceptance will replace mere law-recognition and enforcement.

Through the ages, movements tending toward establishment of government and control of human relations have had two main objectives; on the one hand, the privileges of liberty; on the other hand, exercise of power. Both factors are essential, both involve great dangers. Extent of the danger widens with limitation of either wisdom or knowledge. In organization of human thought religion has been in large measure based upon recognition of power. Philosophy has represented search for knowledge and the sources of power. Science has been the great accumulator of facts. It now

becomes an interpreter, fitting itself closely to philosophy and religion, and serving the needs of government.

In all evolutionary or developmental processes concerning life the principle of coöperation has come to be essential. In development of the organism it is involved in that differentiation making possible the vastly complicated individual with multifarious functions. In society it is at the foundation of those principles which guarantee to us the right of individual achievement limited only by recognition of comparable opportunity for others.

The principle of coöperation in the universe of human beings involves not merely all men in all places, but all men at all times. It recognizes not alone the inter-locking interests of individuals at a given moment, but includes as well the continuity of effort extended through time. It guarantees to the past the value of its contribution. It safeguards the joys of accomplishment of the present. It lays for the future those foundations upon which further building must rest.

The intertwining of philosophic and humanistic interests in the Philosophical Society helps not merely in deepening our perspective in the field of knowledge. It serves also to make more clear the meaning of development of human interests through time. In a day of natural and proper specialization such an organization serves as stabilizer and a means for helping to make more clear the relation between man and his environment, and the place of the individual in the scheme of human organization.

And so, the "Sister Societies" have found represented here their many interests, from laws of atom and molecule to principles of life and religion, each fitting itself into a scheme interpreted in terms of human interest and conduct. The two hundred years of history behind us have seemed perhaps to make more wide the gaps between our various departments in the scheme of knowledge. Before us lies an opportunity for service in picturing through synthesis of these divergent elements, a universe of nature and of human life more wonderful than that which we have known and destined to give us ever-increasing joy of living.

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